ESSENTIALS OF SCIENTIFIC METHOD

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ESSENTIALS

OF

SCIENTIFIC METHOD

BY

A. WOLF, M.A., D.Lit.

Professor of Logic and Scientific Method in the University of London;
Fellow of the University of London, University College;
Formerly Fellow of St. John's College, Cambridge;
Author of "Studies in Logic,"
"Spinoza," etc.

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PREFACE

THIS little book is intended to give an up-to-date, concise account of the aim and methods of science. It may claim to describe and illustrate more scientific methods than are dealt with in any other book; and to present them without those unnecessary encumbrances which so frequently prevent the student from seeing the wood for the trees. *Some* psychological and philosophical considerations are inevitable in a book of this kind; but I have deliberately resisted the temptation of letting the main theme lose itself in a mass of discussions which, though interesting and important, may well be deferred for separate, subsequent consideration. It is my intention to publish, in the near future, two companion volumes dealing respectively with (1) the essentials of deductive reasoning, and (2) the essentials of the philosophy of science. The student of scientific method is strongly advised to supplement his general study of the subject by ample practice in the analysis of the methods employed in actual scientific investigations. He might begin with the analysis of such summaries of scientific researches as are contained in my *Exercises* (pp. 38 ff.), and go on to scientific journals, monographs, etc.

My old friend, Dr. A. T. Shearman, has read the typescript of this book, and I wish to thank him warmly for his friendly help.

A. WOLF.

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ESSENTIALS OF SCIENTIFIC METHOD

CHAPTER I

INTRODUCTION

§ 1. *Knowledge and Life.*

"In the beginning was the deed." Life needs action for its maintenance. One must do things in order to live. In the lowest forms of life the actions are blind and immediate, and their success is not great. The mortality among the lowest types of animals is enormous. But, as we ascend in the scale of animal existence, it becomes more and more possible to avoid many risks by the help of far-sight and fore-sight. Of such far-sight and fore-sight scientific knowledge is the highest known development. "Knowledge is power."

The humblest kinds of animals have no specialized sense-organs, and when they seek satisfaction of their needs they do so with their whole body, which is thus exposed to the risk of injury or destruction. Somewhat more developed organisms, which possess tentacles, are already at an advantage. They can examine their immediate surroundings with their tentacles alone, without risking their whole body. The possession of a special sense-organ of smell

gives a still greater advantage. The animal with a sense of smell has a wider range than is possible with tentacles alone, and need not risk even its tentacles. Special sense-organs of sound and of sight render possible a more real far-sight and fore-sight. Animals so equipped cease to be entirely dependent on what is within immediate reach; they can fetch what they want from afar. They can also realize more distant sources of danger, and seek protection in time. Now human thought and human knowledge extend enormously the range of far-sight and fore-sight. Human beings can satisfy their needs by having recourse to things that are very distant, and they can prepare to meet contingencies which are very remote. Scientific knowledge represents the highest achievement in these respects. Properly utilized it should be the most potent means of protecting the human organism from danger, and supplying all that is necessary for its healthy survival.

Even the lower animals, however, play as well as toil. Their movements and other activities are not always directed to the mere satisfaction of pressing physical needs. Activities are sometimes carried out from the sheer pleasure of action. Such play may be useful in keeping them fit, and in making them expert in the execution of necessary activities. Similarly, human beings sometimes take up sport and athletics with the deliberate aim of keeping fit and agile. For the most part, however, we take pleasure in such activities for their own sake, and without regard to ulterior practical considerations.

So it is with human knowledge. When the conditions of life improve so that it ceases to be necessary to devote our whole thought and energy to the practical needs of existence, then knowledge comes to be pursued for its own sake, without regard to utilitarian considerations. In that way pure disinterested science arises.

Even science in its beginnings was intimately connected with practical needs. Geometry, for example, grew out of the practical needs of the surveyor, biology and chemistry grew out of the practical needs of the medicine man. Even now science subserves practical interests, not only in the sense that, sooner or later, practical applications are found for the purest theories of science, but also in the higher sense that scientific knowledge helps man to make a proper orientation, to take his right place in the world, and to feel at home in it. In this way science, on the one hand, and philosophic reflection and æsthetic contemplation, on the other hand, render possible a more complete and more satisfactory orientation than would otherwise be attainable.

The more speculative flights of philosophic and theological reflection also aim at the satisfaction of certain human needs. But these speculations are apt to be remote from observable reality, as is apparent from the enormous variety of such speculative ideals. It is one of the great services of science that it helps to direct and control such speculative adventures by keeping as close as possible to actual experience. Hence the agreement which one finds

among men of science at all times in comparison with what is prevalent among philosophers and theologians. This is due to the fact that science seeks knowledge along certain well-defined lines. The problems attacked by philosophy are not amenable to solution along the same lines. That is why philosophic solutions are more dubious. But the results of strictly scientific inquiry set certain limits to philosophic speculation, and so help to keep philosophical hypotheses within the realm of probability.

Another great service, perhaps the highest service, which science renders, consists in the cultivation of a certain mental attitude, and in the teaching of certain methods. Both these have proved of inestimable value in the work of scientific research, and may prove equally fruitful even in such problems of life and conduct as do not strictly come within the domain of science. Huxley embraced it as one of the principal aims of his life's endeavour "to forward the application of scientific methods of investigation to all the problems of life," in the conviction that there is no other way of alleviating the sufferings of mankind. It is the object of the following pages to explain, and to illustrate, the character of the chief scientific methods, in the belief that an acquaintance with these methods may be of interest and of use to all intelligent people.

§ 2. *Science: its Aims and its Characteristics.*

Whatever use scientific discoveries may be put to, science as such is a species of theoretical knowledge,

as opposed to all forms of active skill or practical wisdom. Science as such is not an art, or a craft. It is true that scientific experimentation often calls for a considerable degree of technical skill in the construction of suitable apparatus. The discoverer of scientific truth is often also the inventor of scientific instruments, or scientific apparatus; but the technical inventions which pave the way for scientific discovery, and the technical inventions for which scientific discovery lays the foundations, can always be distinguished from science proper, or pure science. Pure science consists essentially of theoretical knowledge.

Not all theoretical knowledge, however, is science. Science is a definite species of theoretical knowledge. There are other branches of knowledge from which science must be distinguished. The expression "science" is mostly used as a collective name for the several sciences—physics, chemistry, botany, etc. These sciences have certain characteristics in common which differentiate them from other departments of knowledge. The common characteristics of the sciences properly so called may be enumerated as follows:—

(*a*) Critical discrimination;
(*b*) Generality and system;
(*c*) Empirical verification.

A brief explanation of these common characteristics of the sciences may suffice for the present purpose.

(*a*) *Critical Discrimination.* The first requisite of all sound knowledge is the determination and the

ability to get at the naked facts, and not to be influenced by mere appearances, or by prevalent notions, or by one's own wishes. Such a mental attitude is commonly described as a scientific frame of mind. It is a *sine qua non* of all science. He who is credulous enough to take things at their face value, or is so lacking in independence and initiative that he cannot break away from customary ideas, or is so partial as to be influenced by his desires and wishes, has not the making of a man of science. Perhaps the best account of a scientific mind is to be found in Francis Bacon's flattering description of himself in his *Proemium*: "A mind nimble and versatile enough to catch the resemblances of things (which is the chief point), and, at the same time, steady enough to fix and distinguish their subtle differences; . . . endowed by nature with the desire to seek, the patience to doubt, fondness to meditate, slowness to assert, readiness to reconsider, carefulness to dispose and set in order; and . . . neither affecting what is new nor admiring what is old, and hating every kind of imposture."[1] Critical discrimination is indispensable in science, but it is really the requisite of all sound knowledge, and is not the monopoly of the man of science. The philosopher and the historian exercise it as well as the scientist. The scientific frame of mind on the part of an investigator is not by itself sufficient to make the results of the investigation a science.

[1] *De Interpretatione Naturae Proemium*, vol. iii. pp. 518 f., in Ellis and Spedding's edition of Bacon's *Works*.

(*b*) *Generality and System.* Science is not interested in individual objects, or in individual groups of objects, as such. It is primarily concerned with types, kinds or classes of objects and events, of which the individual object or event is treated merely as a specimen or an instance. The aim of science is to trace order in Nature. To this end, science seeks to ascertain the common characteristics of types of objects, and the general laws or conditions of events. Each law discovered is, so to say, a thread in the essential nature of the class of objects, or events, concerned; and the discovery of many such laws leads to a conception of the whole pattern or system. In these respects history, that is social and political history, is not a science. It is just as interesting and legitimate a study as science is, and calls for the same kind of constructive imagination and critical insight, but it is different from science. Even the history of science, although it requires considerable scientific knowledge, is a history and not a science. History is concerned with *particular* nations, or institutions, discoveries, or inventions, not with laws relating to nations and institutions, etc., generally. Such general laws would belong to ethnology, or anthropology, or sociology, or psychology, which are sciences, not to history.[1] Astronomy and geology may, at first,

[1] Natural History, of course, is not a history in the present sense of the term. The name "natural history" is a survival from the time when the name "history" was still used for the descriptive account of anything. Aristotle's treatise on Zoology was called *The History of Animals*; and Bacon called all sciences "histories."

appear to be concerned with particular objects; and, to some extent, they may be regarded as marking a transition stage from sciences concerned only with what is general to studies concerned only with particulars. Strictly speaking, however, even astronomy and geology are largely or mainly concerned with what is general. Each stellar orbit is really a law of the sequence of positions of a planet or comet, etc.; and astronomy is also concerned with the formulation of the cycle of stages through which all stellar systems pass. Similarly, geology is concerned with the general relationships between various kinds of strata, and seeks to formulate the sequence of stages through which all continents pass.

(c) *Empirical Verification.* Science begins with facts of actual observation, and constantly returns to observations, in order, directly or indirectly, to check all its tentative explanations, or hypotheses. A suggested explanation which cannot, directly or indirectly, be put to the test of observation, so as to be either confirmed or confuted by it, is of no use in science. In this respect science is different from philosophy. In philosophy it is permissible and usual to put forward hypotheses which cannot be put to the crucial test of observation. True, even philosophical hypotheses are based on experience, and are intended to explain experience; but that is a different thing from being capable of confirmation, or confutation, by observation or experiment under specified conditions. The scientific hypothesis must not only account for all the observations already made of the phenomenon concerned, but must be

capable of being definitely confirmed or confuted by further observations, or experiments, under specified conditions.

§ 3. *Scientific Methods, Technical and Logical.*

In order to obtain the kind of impartial, well-founded, and systematic knowledge at which the sciences aim, certain modes of investigation are followed, which are known as Scientific Methods. In a wide sense, any mode of investigation by which the sciences have been built up and are being developed is entitled to be called a scientific method. Broadly speaking, these methods are of two distinct kinds. On the one hand, there are the *technical or technological methods* of manipulating and measuring the phenomena under investigation, and the conditions under which they can be observed fruitfully. Probably it is these technical methods of manipulation and measurement that are most readily recalled by the expression "scientific methods." These technical methods are mostly different in the different sciences, and few men of science ever master the technical methods of more than one science or one group of connected sciences. On the other hand, there are the *logical* methods, that is to say, methods of reasoning according to the nature of the data obtained. These logical methods are intimately connected with the technical methods. In a very real sense the technical methods, although they are extremely important or even indispensable in many scientific investigations, are mainly auxiliary to the logical

methods of science. What is meant is this. In pure science the technical methods of science are not usually an end in themselves. They are aids either to observation or to inference. Sometimes they render possible the observation and measurement of certain phenomena which either could not be observed and measured at all otherwise, or could not be observed so well and measured so accurately. At other times the technical methods enable the investigator so to determine the conditions and circumstances of the occurrence of the phenomena which he is investigating that he can reason about them in a definite and reliable manner, instead of merely speculating about them vaguely. (For an illustration, see, e.g., Chapter V. § 3.) The conjectural, highly speculative character of early science was probably due, in large measure, to the lack of suitable technical methods and scientific apparatus. However, whereas the technical methods are, for the most part, different from one science to another, the logical methods are more or less common to all the sciences. They are, moreover, the only scientific methods that can be studied with advantage by those who are not men of science, in the strict sense of the term, as well as by those who are. And it is these logical methods of science that will be dealt with in the following pages.

CHAPTER II

THE CHIEF COGNITIVE PROCESSES IN THE SERVICE OF SCIENCE

§ 1. *Observation and Inference.*

Science is the creation of man. Nature, with all her regularities and irregularities, might have been just as real even if there were no men to observe and to study her. But there could have been no *science* without human beings, or beings like them. It is the spirit of man brooding over the stream of natural events that has given birth to science. For science is knowledge, and knowledge is the result of mental activities operating upon a world of objects. Now, speaking generally and without any attempt at psychological analysis at this stage, the mental activities which lead to scientific knowledge are roughly of two principal kinds, namely, processes of *Observation*, and processes of *Inference*. By Observation is meant the act of apprehending things and events, their attributes and their concrete relationships, also the direct awareness of our own mental experiences. By Inference is meant the formation of judgments (beliefs or opinions) on the strength of, or as a consequence of, other judgments already formed, it may be, on the ground of observations, or only entertained provisionally either for

further consideration, or for the sake of argument. The broad distinction between observation and inference is sufficiently clear. But it is not always easy to draw the line between them, as will be shown, to some extent, later. And, unfortunately for clear thinking, people do not always realize that they are drawing inferences when they pass from particular observations to generalizations or to forecasts.

In the case of observation in the interests of science we may distinguish two principal kinds, namely: (*a*) *bare observation* of phenomena under circumstances which are beyond control, and (*b*) *experiment*, that is observation of phenomena under conditions which the investigator can control. Bare observation, as also experiment, may be assisted greatly by the use of scientific instruments, such as telescopes, microscopes, etc., also by selecting specially suitable times and places for making the observations; but no scientific instruments, and no amount of trouble taken over the observation of the phenomena investigated, can be said to render the observation experimental in character unless the phenomenon observed and the circumstances of its occurrence are actually affected and controlled thereby. The chief advantage of experiment over bare observation is, that under experimental conditions it is usually easier to analyse accurately a complex phenomenon into its components, and to vary the circumstances of its occurrence in such a way that it is possible to draw reliable inductive conclusions concerning the connection between

certain antecedents and consequents, or conditions and results. When phenomena and the circumstances of their occurrence are entirely beyond the control of the investigator, he is apt to overlook some important factors altogether, and to misjudge the function of others.

Inference, likewise, has two chief types, namely, *Induction* and *Deduction.* Inductive inference is the process of ascertaining some kind of order (class-character, law, or regularity) among the phenomena observed and studied. Deductive inference is the process of applying either inductive conclusions or hypothetical concepts, laws, or regularities to suitable cases or classes of such cases. In the scientific study of natural phenomena, inductive inference plays the most important rôle, though deductive reasoning also contributes its share. Although deduction is by no means as easy as is sometimes supposed, still it is certainly easier than induction. For one person who can make an inductive discovery, there are thousands who can apply it deductively. Anyway, deductive reasoning is dealt with in Deductive or Formal Logic. The special study of scientific method is mainly concerned with the character of the principal types of valid inductive inference, each of which constitutes a scientific method.

Observation and inference, however, are very complex processes, and it is advisable to consider some of the constituent processes which are of the utmost importance for all scientific investigation, but cannot be described as specific methods of science

because they are really constituents of all, or nearly all, scientific methods properly so called. The cognitive processes referred to are those of analysis and synthesis, imagination, supposition and idealization, comparison and the perception of analogies. A brief account of these various processes follows; but no significance should be attached to the order of their exposition in the following sections.

§ 2. *Analysis and Synthesis.*

The discovery of order in the phenomena of nature, notwithstanding their complexity and apparent confusion, is rendered possible by the processes of analysis and synthesis, which are the foundation of all scientific methods. The objects and events which we observe are nearly always complex, but, mentally at least, we can always analyse them into their constituents or components. This process is helped by the comparison of two or more objects or events which are similar in some respects, and different in others. But in its turn analysis facilitates more exact comparison. Having analysed the complex whole into its parts or aspects, we may tentatively connect one attribute of a thing with another, or one aspect of a thing with another, in order to discover a law; or we may, in imagination, synthesize again some of the attributes or aspects, and so form an idea of what is common to many objects or events. The process may be extended to classes of classes, whether of things or of events.

The elements obtained by the analysis of different objects or events may also be synthesized in such a

way as to form combinations the like of which have never been observed at all. In this way we form or acquire general and abstract ideas without which all higher knowledge, including science, would be impossible. In some cases, as in physics, chemistry, etc., the processes of analysis and synthesis can also be carried out materially (that is, objects can actually be broken up into their parts, or *vice versa*), and then scientific discovery may be greatly aided by the experimental variation of the conditions of the phenomena, in accordance with the direct methods of induction.

§ 3. *Imagination, Supposition, and Idealization.*

The presence of order in nature is not very obvious. The impression made by the observation of natural phenomena is, for the most part, one of bewildering confusion. How do we come to look for order at all? Why do we take the trouble to discover classes, connections, or laws? The answer is fairly clear if we bear in mind the original character of human knowledge, including science. Knowledge was born in the service of life; it was, and in many ways still is, essentially an instrument of life; and life needs some kind of order in its environment, if it is not to be a perpetual groping in the dark.

Where possible life actually creates order of some degree, as may be seen in the habits of animals, in the customs, laws, and conventions of human society. In the case of natural phenomena, of course, man cannot *create* order, he can only look for it, try to discover it, if it is there. But it is the

need for order, as an aid to life, that prompts man to search for it everywhere. If he succeeds, well and good; if he fails to discover order, or some special form of order, in any realm of facts, then he modifies his expectation, or maybe abandons it altogether, and turns his attention to other facts in the hope of discovering order there. Sometimes, indeed, the need for perfect order has been felt so keenly, in the face of the obdurate disorder of an imperfect world, that men like Plato and other idealistic philosophers have conceived an ideal, transcendental world over and above this world of ours. Here is shown, in an extreme form, the felt need for order which, in varying degrees, all intelligent human beings experience. And it is this felt need that always prompted mankind, and still prompts us, to try and discover order in Nature.

The actual search for order in nature follows more or less the usual modes of human conduct. It begins with what is known as the method of trial and error, and, in the course of time, is characterized more and more by that insight and guidance which are the fruits of accumulated knowledge and experience. That is to say, at first, any kind of classification might be tried, based on any kind of resemblances, and any facts or events may be believed to be connected, if they are observed to be conjoined. But, in due course, the mistakes are corrected in the light of subsequent experience, and the process of discovering order is carried out with far greater caution, with the help of, and with increasing regard for, the knowledge already

acquired. Such a cautious, tentative attempt to discover order in any group of facts, by trying to fit a supposition that would make them appear orderly, constitutes what is known as the method of hypothesis.

An hypothesis is any tentative supposition by the aid of which we endeavour to explain facts by discovering their orderliness. All the methods of science may be said to depend on fruitful hypotheses. So long as it can be put to the test, any hypothesis is better than none. Without the guidance of hypotheses we should not know what to observe, what to look for, or what experiments to make, in order to discover order in nature. For observation not guided by ideas, even hypothetical ideas, is blind, just as ideas not tested by observation are empty.

Hypotheses or suppositions are, of course, used in everyday life, and in philosophy and in theology, as well as in science. In science, however, no hypothesis is seriously entertained unless it can be put to the test of observation, either directly or indirectly. Hypotheses may, of course, be true even if they cannot be tested or verified. On the other hand, hypotheses that can be tested by observation frequently turn out to be false, when so tested. Nevertheless, science has no use for *barren* hypotheses, that is, hypotheses which cannot be put to the test. Many hypotheses, which subsequently turned out to be false, were fruitful all the same, because they suggested lines of investigation which, though they led to the repudiation of those hypotheses, also led to the discovery of truths. But

hypotheses which are barren at one time may become fruitful at a subsequent period, when suitable scientific instruments and processes have been invented. Thus, for example, most of the hypotheses relating to air were barren until Guericke invented the air-pump, and the chemists of the eighteenth century invented suitable processes for the analysis of air.

Intimately connected with the processes of imagination and supposition is the process of idealization, whose function in science has not hitherto met with due recognition. By idealization in science I mean the process of conceiving the ideal limit of some phenomenon that has been observed in various forms more or less approximating that limit, but never reaching it. The conception of ceaseless movement, implied in the first law of motion, is a case in point. The numerous uses of limiting cases in mathematics, and the conception of a purely "economic man" in economics, are other instances. When supplemented by the process of hypostasization, that is, the process of treating an idea or a concept as though it were an existing object, the process of idealization may go a long way to explain the ideal constructions of mathematics, without our having to resort to a Platonic idealism or a Scholastic realism.

§ 4. *Comparison and Analogy.*

The observation of similarities and differences, aided by the processes of analysis and synthesis, constitutes one of the first steps in all knowledge,

and accompanies its progress throughout. But there are degrees of similarity. Things, attributes, or events may be so similar that we regard them as being of the same kind, or as belonging to precisely the same class or type. On the other hand, there is a similarity which stops short of such close class-resemblance, and then we refer to it as analogy. In its wide sense the term analogy is applied to similarity of function, similarity of relationship, in fact, almost to any similarity short of that which characterizes members of the same class of things or events. Now, analogy also plays an important rôle in the advance of science. The acquisition or discovery of new knowledge is rendered possible by utilizing the knowledge already acquired. It is a process of apperceiving new or strange phenomena in the light of what is already known of other similar or analogous phenomena. In our search for order in any group of phenomena we naturally attempt to "try on" any kind of order with which we are already familiar. Hence, analogy is a very fruitful guide to the formation of hypotheses or tentative orders of phenomena. Sometimes, indeed, what at first appeared to be a somewhat remote similarity or analogy may, on further investigation, turn out to be so close that what at first appeared new and strange is included in the same class as the old, by the aid of which it was apperceived. Thus, for example, lightning turned out to be the same kind of thing as an electric flash, and the movement of the moon was shown to be the same kind of phenomenon as the fall of an apple. This result,

it is true, is not very common; but even in other cases analogy is very helpful. One need only think of the most important discoveries in the history of science, in order to realize the enormous value of analogy. Our conception of the solar system (the helio-centric theory) owes a great deal to the analogy of the miniature system of Jupiter and the Medicean satellites. Some of the most important discoveries in modern mathematics are due to the analogy, discovered by Descartes, between algebra and geometry. The wave-theory of sound was suggested by the observation of water-waves; and the undulatory theory of light was suggested by the analogous air-waves which transmit sound. The theory of natural selection by the struggle for existence was suggested to Darwin by his knowledge of the artificial selection by which breeders have produced the many varieties of domestic animals. And so forth.

It is important, however, to bear in mind also that analogy, as suggested, is not an independent scientific method but only an aid to the formation of hypotheses. Its sole service consists in originating hypotheses, and so suggesting lines of research in which scientific methods may be employed. By itself, analogy establishes nothing, notwithstanding the frequent reference one meets with to what is called "proof by analogy." The reason may be briefly indicated as follows. Generally speaking, what happens in so-called proof by analogy is this: some phenomenon or a class of phenomena, say *S*, is observed to resemble some other phenomenon or

class of phenomena, say Z, in respect of some feature, say M; from this similarity it is concluded that S resembles Z also in respect of some other feature, say P, which Z is known to possess, but which has not yet been observed in S. Now, such a conclusion can only be justified if it can be shown that, directly or indirectly, M and P are connected by some law, for unless indeed there is some ground for supposing that M and P are connected in some way, the similarity between S and Z in respect of M is really irrelevant in considering their possible similarity in respect of P. But the question of the connection between M and P can only be decided by the inductive methods, not by the mere analogy. For example, the undulatory character of the transmission of light and sound, as already remarked, was suggested by the wave-motion of water; but only *suggested*. The hypothesis had to be verified by observation and experiment. If analogy alone were sufficient to warrant a conclusion it might have been assumed that, since the transmission of sound is analogous to that of light, the phenomena of polarization, which are found in the case of light, would also be met with in the case of sound. But analogy could only *suggest* this as an hypothesis, which subsequent inductive investigation has *not* verified. So that analogy, like all perception of similarity, and like analysis and synthesis, and imagination and supposition, must be regarded as an auxiliary or a preliminary to the inductive methods properly so called, rather than as an independent scientific method.

CHAPTER III

CLASSIFICATION AND DESCRIPTION

§ 1. *Classification.*

Science, like all human knowledge, begins with sense-experience. But sense-experience is so diverse and so complicated as to appear almost chaotic. In a real chaos life, or at least a rational life, would be impossible. So from earliest times the human mind sought out the elements of order in the world, and the first step in this direction consisted in the noting of similarities between things. Such noting of similarities between things constitutes an implicit, if not an explicit, classification of them. At first, no doubt, classification subserved strictly practical purposes. Similar objects, or events, were simply such as could be treated as equivalents one for another for certain practical purposes. In course of time, however, as human knowledge gained some freedom from the bonds of immediate needs, increasing disinterestedness was shown in the similarities observed. Classification then tended to become more and more objective, or more and more natural, attention being paid more to the character of the things themselves, instead of to their human uses.

The vast number of classifications spontaneously made by early man is obvious from the evidence of

language. Every name expresses the recognition of a class of objects; and language is much older than science. Many of these early classifications were based on inadequate observation. Superficial resemblances often succeeded in concealing deeper differences, or superficial differences succeeded in disguising more important similarities. Deliberate reflection and scientific study have, therefore, ample opportunity to correct the classifications which are implicit in language. For example, in popular language and thought a whale is just a fish because it lives in water, coal is just a mineral because it is found in a mine, and a sea-anemone is a vegetable because it rather looks like it. For science, on the other hand, a whale is a mammal, coal consists of fossilized plants, and a sea-anemone is an animal. The idea that a gas might be a metal, or that the processes of breathing, burning, and rusting belong to the same class of events, would be altogether beyond popular conceivability, but it is the idea put forward by science. Notwithstanding such differences, however, the popular classifications implicit in language usually form the starting-point of scientific classification. Scientific classification hardly ever begins from the beginning, but rather sets out from current classifications. Sometimes even men of science may feel uncertain about the proper place of certain things in the recognized schemes of classification, and, in that case, it may be best to admit a new class. That is what happened, for instance, in the case of the single-celled creatures now known as *Protista*. Some

biologists classed them with vegetables, others with animals, but eventually they were recognized as a distinct intermediate class, and called *Protista.*

Classification, then, is in some ways the earliest and simplest method of discovering order in nature. To recognize a class is to recognize the unity of essential attributes in a multiplicity of individual instances. Classification is thus a recognition of the one in the many. The method of classification is the first method employed in every science. Long before there is that deeper insight into facts, which is required for the more advanced methods of science, the method of classification can be, and has to be, employed. Many sciences, indeed, remain for a long time in a merely classificatory stage, and have consequently come to be known as classificatory sciences. This is especially true of botany, zoology, and ethnology, and was at one time almost equally true of chemistry, mineralogy, and some other sciences.

Classification is a method of science, it is a way of knowing or regarding things. It is primarily an intellectual activity, not a physical activity. The classification may be exemplified or illustrated by the grouping of objects in a museum, for instance. But the physical grouping is not the real, essential classification : it is only an illustration of it. The essence of a classification consists in the fact that certain things are thought of as related in certain ways to one another. The things may be, and usually are, too numerous to be physically grouped together, and, even if an actual physical arrange-

ment were possible, such an arrangement would be based on a prior intellectual classification, and would not itself constitute a real classification. Classification is a mode of knowledge, a way of grasping the unity of certain things, and the relation between various kinds of things. Classification has, of course, an objective basis in the actual kinship, or similarity, of the objects classified. The man of science is not supposed to invent or to create, but only to discover the sameness or similarity of character in the things, processes, etc., which he classes together. But sameness of character is something very different from a physical grouping together of objects. The arrangements seen in a botanical garden, or in a zoological garden, or in a natural history museum, or in a museum of ethnology or of mineralogy, are not classifications in the strict sense of the term, but arrangements *illustrating* classifications.

The aim of scientific classification is to see things according to their actual objective relationship. Such a classification is what is meant by a *natural* classification. There are other classifications. It has already been pointed out that the earliest classifications tended to be based on man's practical needs in relation to the objects concerned. Even long after that stage was passed, classifications were, owing to insufficient knowledge, based on merely superficial, or less important, attributes. The history of botany, for example, is, to a large extent, the history of various attempts to classify plants on the basis of all kinds of attributes, such as the

character of the leaves, of the fruit, or of the corolla, of the calyx, or of the stamens. What is sought after in natural classifications, especially in biology, is that the things as a whole should be taken into account, with all their important attributes. To achieve this satisfactorily a great deal of knowledge is usually required. Often, indeed, such knowledge can only be acquired with the aid of the higher scientific methods. The method of classification, therefore, although it is the first and the earliest method of science, may also, in a sense, be the last method of science, for the final outcome of the application of other methods of science to certain classes of facts may be a new classification of those facts. This may be seen to some extent in the recent history of biology and of chemistry.

Classification is not only of individuals into classes, but also of classes into wider or higher classes, and of those into still higher classes. In so far as the ideal is attainable, the facts investigated in a science can be conceived as members of a perfectly orderly scheme of things. All classifications are based on the presence or absence, or the presence in varying degrees, of certain attributes; and those classifications are the most natural in which the attributes selected as the bases of the classifications are such as carry with them the presence or absence, or the presence in varying degrees, of other attributes. Mammals, for example, are usually classified according to the character and arrangement of their teeth, because agreement and difference in these respects are found to be cor-

related with agreement or difference also in other respects. For similar reasons the classification of the chemical elements is based on their atomic weight, with which their specific heat, also their boiling point and melting point, are usually correlated, at least within the same periodic group of elements.

Classifications made for special, practical purposes are usually called *artificial* classifications. They are not made from the standpoint of the objects themselves, so to say, but from the standpoint of the practical needs of man. For example, the usual classification of plants, found in standard treatises of scientific botany, is a natural classification. It is based on what is believed to be the objective or natural kinship of the plants themselves and is not intended merely to serve some practical purpose of man, except to satisfy his desire for knowledge, for pure science. But the druggists' and herbalists' classification of plants is different. These classifications have reference primarily to the needs of man for medicinal remedies. Such an artificial classification is perfectly legitimate, and in a sense even natural, namely, in the sense that it is based on *some* objective character of the plants, and that it is suitable for the purpose in view. But it is artificial, and not natural, in so far as the basis on which it rests is in the main something extraneous to the plants themselves.

The more restricted the purpose for which the classification is made, the less informing is it likely to be about the essential objective character of the

objects so classified. An extreme case of this may be cited from *Punch*. An old lady went on a railway journey with a menagerie of pets. The railway porter told her what the fare would be for her dogs, but did not know the tariff for her other pets, so she sent him to the station-master to inquire. When he returned he said: "Station-master says, mum, as cats is dogs, and rabbits is dogs, and so is parrots, but this 'ere tortoise is a hinsect, so there ain't no charge for it." From the limited view point of the railway company's schedule of fares, dogs and cats and rabbits and parrots all belong to the same class. This classification was not made in the interests of science, and what is done for a special purpose can only be judged in the light of its suitability for that purpose, and not by the impersonal standard of pure science.

§ 2. *Description, General and Statistical.*

Classification is intimately connected with description. When objects are recognized as forming a class, the class has to be named and described. The name and the description help to make permanent the result of the process of classifying. They facilitate future reference to the subject on the part of the discoverer, and they make it possible to communicate the discovery to others. Science is essentially the result of co-operation. Scientific workers in any field of inquiry keep in touch with one another through the medium of scientific societies, scientific periodicals and other publications. They exchange ideas and check one another's

results. In this way what is likely to be true for all is sifted from the mistakes of the individual.

Now, description may be comparatively easy and simple, or it may be difficult and complicated. In a simple case a statement of easily recognizable parts, qualities and processes might suffice; in a more difficult case the parts, qualities and processes may be hard to describe, and precise quantitative considerations may be involved besides. The sciences have, accordingly, developed nomenclatures (or systems of names for all the classes of objects with which they are concerned), and terminologies (or systems of expressions, including names, verbs and adjectives, for the parts, qualities and processes of the individual objects included in the various classes), and certain statistical devices for the most convenient and most informing expression of the quantitative aspects of the things within their domain. The terminological schemes and the statistical methods are important aids to description. The description is, of course, a description of things; but the concise, economic description of the things in a class constitutes the definition of the name of the class.

The description of the objects included in the same class will naturally confine itself to attributes which they have in common, and only to some of these. The attributes selected to be included in the description will be those that are considered to be the most important, in the sense that they actually are, or are likely to be, correlated with more of the remaining attributes than are the others.

With the progress of knowledge our estimate of the relative importance of the different attributes may change, and the descriptions or definitions are, therefore, liable to revision. When, as frequently happens in geometry, there are several equally correct ways of describing concisely the same class of objects, then the selection of one of them in preference to the others may be guided by considerations of convenience. That description or definition will be preferred which enables the reader, or listener, to realize most easily the nature of the objects described. That is the reason why in geometry the different classes of rectilinear figures are described by reference to the sides, and not by reference to the angles, although the names of some of them actually have an obvious etymological reference to the angles rather than to the sides, for example, triangle, rectangle, pentagon, etc. In addition to such concise descriptions, which are used in the scientific definitions of the names of the classes, there are other descriptions in use as well; also typical pictures and diagrams, when possible, in order to convey a vivid idea of the kind of thing described, even to those who have never actually perceived an instance of it.

Science, however, aims at exactness, and is not satisfied with anything that is more vague or indeterminate than is necessary. Now no two things are exactly similar, and, in order to place objects at all in the same class, many individual differences have to be ignored, emphasis being laid on the common attributes. But that does not yet

get over all the difficulties. Things belonging to the same class may be of the same kind, in the sense that they bear a general resemblance to one another in important ways, yet they may vary, nevertheless, from one another even in respect of some of these similar features. This is especially true of living objects. Plants and animals of the same species vary from one another in sundry ways. Such variations are not something abnormal, but something quite common, in biology. The exact description of the class must consequently take cognisance of these variations, as well as of the resemblances. An example or two may help to elucidate this.

Prawns have dorsal teeth, but the number of dorsal teeth varies from individual to individual. Some have only one dorsal tooth, while others have as many as seven dorsal teeth. How shall the type be described with reference to the number of these dorsal teeth? To answer this question a biologist examined 1,434 specimens, which he collected from an estuary near Plymouth. The examination of their dorsal teeth gave the following results :—

2	had	1	dorsal	tooth
23	,,	2	,,	teeth
103	,,	3	,,	,,
533	,,	4	,,	,,
681	,,	5	,,	,,
89	,,	6	,,	,,
3	,,	7	,,	,,

In a case like this the type might be described by means of some kind of *average*. One might take an arithmetical average or *mean*, adding up the number

of teeth possessed by all the above prawns together, and dividing the total by the number of prawns. The typical number of dorsal teeth would then be about 4·5. Or one might take the *mode* as typical, that is the number which is found most commonly, in this case it would be five, which is the number of dorsal teeth possessed by the members of the largest group. Or one might regard the *median* as typical, that is the value of the individual in the middle of the whole collection, when all the individuals in the collection are arrayed, or conceived to be arrayed, in an ascending or descending order of magnitude. In this case the middle prawn would be the 717th or 718th, and both fall within the group of the 681 with 5 dorsal teeth.

Take another example, clover sometimes has flowers in which one or more florets are higher than the rest. De Vries examined 630 specimens with the following results :—

325	clover flowers	had	0	raised	floret
83	,,	,,	1	,,	,,
66	,,	,,	2	,,	florets
51	,,	,,	3	,,	,,
36	,,	,,	4	,,	,,
36	,,	,,	5	,,	,,
18	,,	,,	6	,,	,,
7	,,	,,	7	,,	,,
6	,,	,,	8	,,	,,
1	,,	,,	9	,,	,,
1	,,	,,	10	,,	,,

In this case the arithmetic average or mean would be about 1·5, while the mode and the median would be 0.

When the type has been determined in one of the above ways it still remains to indicate the extent to which the individuals deviate from the type. The individuals in a class of a given average, or type, may be more or less homogeneous, and the extent of their homogeneity or heterogeneity must be indicated in some way. This is done by measuring the deviation from the average, in simple collections like the above, and ascertaining the typical or average deviation from the average or type. In the case of the prawns, for instance, one would tabulate the differences between five and the actual number of dorsal teeth possessed by all the individual prawns examined, and take either the arithmetical average of these differences, or deviations (called the *average deviation*), or their median (called the *median deviation* or *probable error*). The prawns show a median deviation of one; the typical number of dorsal teeth would, therefore, be represented as 5 ± 1.

The raised florets of the clover-flowers have a median error (also an average deviation) of about 1·5, the same as their arithmetic mean, so that the typical number of raised florets would be expressed by $1 \cdot 5 \pm 1 \cdot 5$.

Another kind of average deviation frequently employed is that known as the *Standard Deviation* (or σ). It is the square root of the average of the squares of the deviations from the arithmetical average of the group. And the expression "*probable error*" (p.e.) is sometimes used conventionally for the standard deviation multiplied by a constant.

CHAPTER IV

THE EVOLUTIONARY AND COMPARATIVE METHODS

§ 1. *The Evolutionary or Genetic Method.*[1]

There are some facts which are not sufficiently similar to be regarded as belonging to the same immediate class, but are similar enough to make us regard them as belonging to neighbouring classes, or as sub-classes of some higher class. In such cases one is sometimes led to suppose that the similar classes are kindred in the more or less usual sense of being descended from a common ancestor or antecedent. A new problem thus arises, namely, that of tracing the stages in the descent or the development of the kindred classes. This is especially true of living objects, their organs, and their functions, etc. In the case of human beings, there are similar problems relating to the origin and the development of their customs, their institutions, and their inventions, etc. The scientific method by

[1] This method is frequently called the Comparative Method (see the next section). Sometimes it is also called the Historical Method. But this name is so ambiguous that it is best to avoid it. Mill applied it to a form of the Deductive-Inductive Method (see Chapter VII). And not infrequently a problem is said to be treated by the Historical Method when all that is meant is that an account is given of the different *views on the problem* put forward by various investigators at different times.

which such developments are traced is known as the Genetic Method, also as the Evolutionary Method; and in so far as the method succeeds in establishing the stages in the evolution of certain classes of facts, a better insight is obtained into their unity and continuity than is afforded by ordinary classification. Sometimes, in fact, the real significance of the resemblances on which the usual classifications are based, or to which they draw attention, is first brought out by the Evolutionary Method, which elaborates a mere table of similar classes into a genealogical tree, develops a merely static classification into a kinetic or phylogenetic scheme. The various classes are then unified or connected very intimately by being shown to be phases or stages of one continuous process, or a system of intimately connected processes.

The science which appears to have been the first to employ the Evolutionary Method is Comparative Philology, which used it already in the eighteenth century. The ground was prepared for its use in comparative philology, inasmuch as people were long familiar with the idea of the unity of mankind, and the existence of a common human language until the time of the Tower of Babel, since when that ancestral tongue, it was believed, had developed into a variety of languages. It was, therefore, felt to be reasonable to compare the different languages in existence, and to attempt to trace the history of their evolution in the light of such similarities and differences as the comparisons disclosed. This very assumption of an historic development in the case

of languages constituted the first requisite for the application of the Evolutionary Method in Comparative Philology.

It was different with Comparative Anatomy. The same Biblical narrative which had facilitated the application of the Evolutionary Method in Comparative Philology hindered its application in Comparative Anatomy. For it taught, or was supposed to teach, that the different kinds of animals had each been created separately, and were thus distinct from one another in their origin. The similarities observed by the early anatomists were, consequently, regarded merely as interesting curiosities, and led no further until the time of Darwin, whose *Origin of Species* is the classic application of the Evolutionary Method in Biology. Only since the publication of this book has Comparative Anatomy really become comparative in the sense of the Evolutionary Method.

The Evolutionary Method is, then, applicable only to those classes of facts which can, tentatively at least, be regarded as the products of a process of development. It is the function of the method to indicate (*a*) the main steps or stages through which the development has probably taken place, and (*b*) the reason for the various changes constituting the several stages in the suggested line of development. When all the stages of the evolution of anything are known from direct observation and record (as in the case of many varieties of fruits, of pigeons, or the stages in the development of the bicycle) there is no occasion to apply the

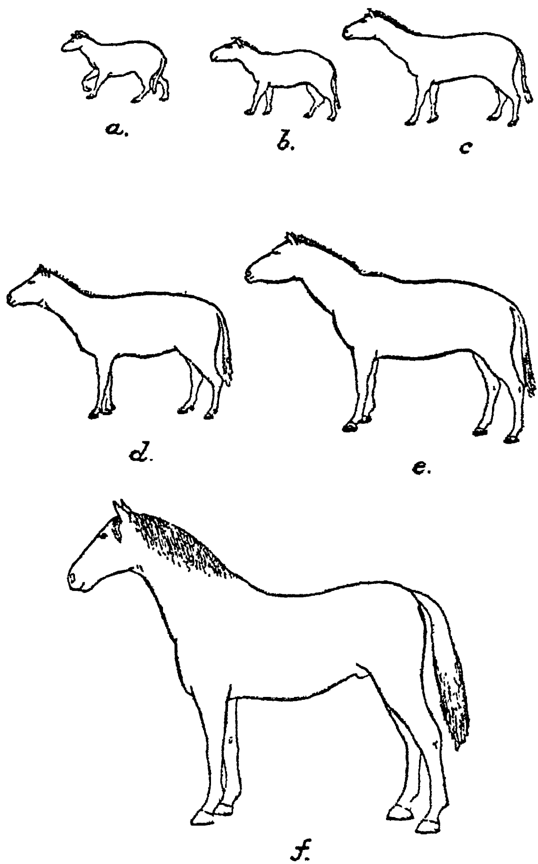
Evolutionary Method. It is only in cases where few of the earlier forms are known that the Evolutionary Method proceeds hypothetically to suggest a probable line of development. Thus, for example, in the case of the horse we have no such detailed knowledge of its descent from a five-toed ancestor as we have of the varieties of pigeons descended from the wild wood-pigeon. Only a few of the alleged intermediate forms of the horse are known, and the Evolutionary Method, basing itself on such evidence, has put forward an hypothesis relating to the probable series of variations through which the horse, as we now know it, has passed.

The whole theory of biological evolution rests on applications of the Evolutionary Method; and all the phenomena to which the conception of evolution is applicable afford opportunities for the application of that method. The method can be applied, and is, indeed, being applied, not only to plants and to animals, to social customs and social institutions, to the human mind, to human ideas and ideals, but also to the evolution of geological strata, to the differentiation of the chemical elements, and to the history of the solar system.

When searching for the gradations through which some product of evolution has passed, the correct thing, according to Darwin, is to look chiefly among kindred classes of objects (animals, organs, etc.). But it is rarely possible to obtain sufficient evidence from a study of the nearest kindred only, and the investigator is, therefore, frequently compelled to go farther and farther afield, among less and less

kindred classes of objects, in search of missing links in the chain of evolution. Thus, for example, Darwin himself, when dealing with the development of the honeycomb of the hive-bee, kept close to kindred species, but, when he traced the evolution of the eye of vertebrate animals, he went far afield, referring to facetted eyes, eyes without a lens, and eyes which are mere collections of pigment cells. Similarly, the comparative psychologist, when tracing the evolution of the human mind, does not confine himself to the primates, or higher apes, but seeks light also among the lower animals.

As an example of the Evolutionary Method we may refer again to the development of the horse. The present-day horse is a large quadruped, has only one toe on each foot, a splint-bone on each side of the upper end of the cannon-bones, and so on. The fossil remains of quadrupeds now extinct have enabled zoologists to reconstruct the history or evolution of the horse, which the accompanying sketches may illustrate. In the Lower Eocene Age there existed a small kind of quadruped not larger than a fox, in some ways much simpler in structure than the modern horse, but having four toes on each foot. In Oligocene times the descendants of these quadrupeds had only three toes on each foot, but were larger in body. In the course of time the middle one of the three remaining toes gradually increased in size, and was alone used in walking and running, while the other two toes became smaller and smaller, eventually remaining merely as splint-bones. Concurrently with these and other changes,



THE ANCESTORS OF THE HORSE AND ITS RELATIVES COMPARED IN SIZE AND FORM WITH THEIR TYPICAL MODERN REPRESENTATIVE.

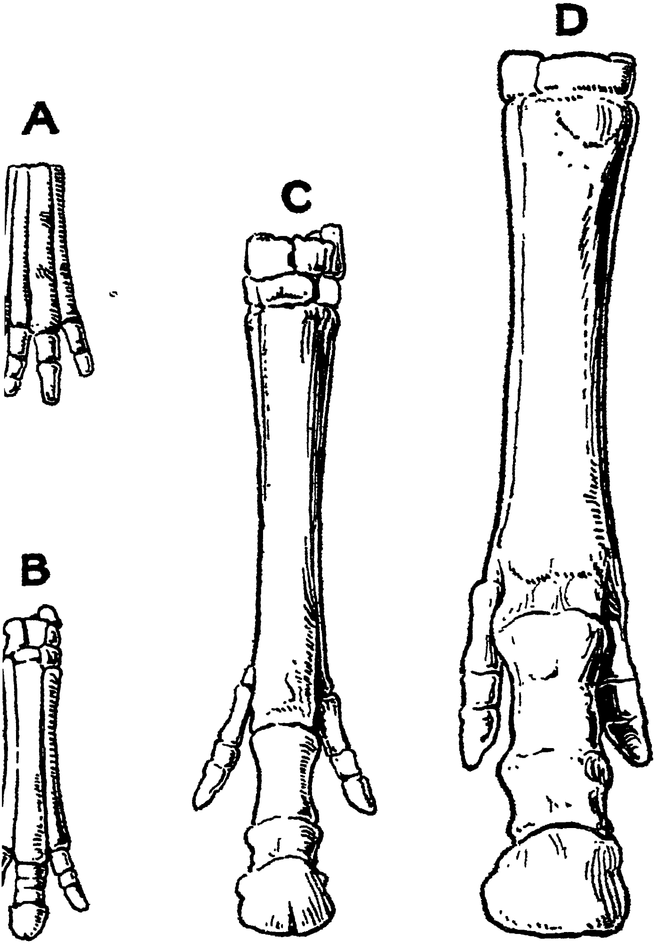
a. *Hyracotherium*, or *Protorohippus*, of the Lower Eocene; b. *Plagiolophus* or *Orohippus*, of the Middle Eocene; c. *Mesohippus*, of the Oligocene; d. *Merychippus*, of the Miocene; e. *Pliohippus*, of the Pliocene; f. The Modern Horse, *Equus caballus*, domesticated breed (Lull, *Amer. J. Sci.*, vol. xxiii, p. 167).

the body of the horse grew in size. More speculatively, the history of the horse is carried a stage farther back to a still smaller ancestor that had five toes on each foot, and walked on the whole sole of its foot instead of on one toe only.

§ 2. *The Comparative Method.*

The term "Comparative Method" is frequently or even usually employed as synonymous with the term Evolutionary Method, explained in the foregoing section. This use of the term is, on the whole, warranted. It has come about in this way. Some sciences have long been known as "Comparative Sciences"—Comparative Philology, Comparative Anatomy, Comparative Physiology, Comparative Psychology, Comparative Religion, etc. Now the method of these sciences came naturally to be described as the "Comparative Method," an abridged expression for "the Method of the Comparative Sciences." And when the method of most comparative sciences came to be directed more and more to the determination of evolutionary sequences, that is to say, became evolutionary in that sense, the term "Comparative Method" came to mean what is now frequently described as the "Evolutionary Method."

The method of the comparative sciences, however, was not always the Evolutionary Method, and is not always so even now. And, in consequence of certain differences among sociologists and ethnologists, the tendency is to distinguish between the Comparative Method and the Evolutionary Method,



SKELETON OF FORE-FEET OF EXTINCT FORE-RUNNERS OF THE HORSE:

. *Hyracotherium* (No. N. H. 65); B. *Mesohippus* (No. N. H. 63); C. *Merychippus*, or *Protohippus* (No. N. H. 57); D. *Hipparion* (No. N. H. 44).

[*uide to the Horse Family*, British Museum (Natural History).]

the latter term being employed in the sense explained in the previous section, while the former is given another meaning. The precise meaning of the term Comparative Method, when it is distinguished from the Evolutionary Method, is not easily determined, as it appears to be used somewhat loosely and nebulously. To say that the Comparative Method is a method of comparison is not illuminating, for comparison is not a specific method, but something which enters as a factor into every scientific method. Classification obviously requires careful comparison; and every other method of science depends upon a precise comparison of phenomena and the circumstances of their occurrence. All methods are, therefore, "comparative" in a wide sense. How, then, does the term Comparative Method come to be used at all in a wide sense, as distinguished from its restricted meaning when it is regarded as synonymous with Evolutionary Method?

The answer is to be found partly in the somewhat special or peculiar circumstances of Sociology, the science of social groups. The most familiar way of studying a social group is that of the historian. Now, the historian gives a chronological account of individual social groups as such; *qua* historian it is not his business to compare a number of social groups with a view to generalizing about them. Sociology, however, being a science, and not a history, is concerned with the discovery of general truths relating to social groups. The sociologist, accordingly, does compare many social groups, to

note their similarities and their differences; he even studies what is known of extinct social groups, or of extinct customs and institutions. In order to stress the *general* character of his study (in contrast with the *particular* character of the strictly historical study of a social group), he describes his science as a *comparative* study of social groups or institutions. Now, the comparison may lead to classification, the classification, for instance, of the main types of social structure, or of the principal forms of human marriage. It may lead to the application of the Method of Agreement (see Chapter V, § 5) in order to establish some causal connection, as, for example, when a comparison of the various circumstances under which the practice of human sacrifice is met with leads Professor Westermarck to the conclusion that the motive prompting it is that of life-insurance, based upon the idea of substitution. Or, again, the comparison may lead to the application of the Joint Method of Agreement and Difference (see Chapter V, § 7), as, for instance, when Dr. Lowie tries to establish a connection between the social system of clan exogamy and the classificatory system of relationship. The other inductive methods may be similarly employed in consequence of such comparisons. And sometimes the comparison of social institutions at different stages of culture may lead to the discovery of an evolutionary sequence or series, and the application of the Evolutionary or Genetic Method. But, it is argued, the comparative study of any such phenomena is not necessarily bound up with the tracing of evolutionary development.

A comparative study of phemomena may be pursued by investigators who do not believe that the phenomena in question are the products of evolution, or who possibly do not believe in evolution of any kind. Hence the need of differentiating between the Comparative Method and the Evolutionary Method.

The foregoing considerations may render intelligible the term Comparative Method as it is sometimes used. This usage, however, is not to be commended. For, according to it, the term Comparative Method is little more than a vague name for *any* scientific method. The main purpose of the sociologists concerned would be attained more accurately, and at least as effectively, if they simply distinguished between *Special* Sociology and *General* Sociology. This would correspond to the common distinction between, say, special anatomy (e.g. human anatomy), or special philology (e.g. English philology), on the one hand, and comparative anatomy, or comparative philology, on the other. In the so-called "comparative" sciences the word "comparative" is probably too well established to be abandoned; not so in Sociology. But even if the term "Comparative" Sociology should be used instead of General Sociology, there is no adequate reason for continuing to use the rather nebulous expression "Comparative Method," instead of specifying the precise methods intended, such as that of Classification, Agreement, etc.

Generally speaking, in so far as the comparative study of biological phenomena does not end in

classification, the observation of similarities does not really explain them, but rather calls for an explanation. For example, the resemblances observed by early biologists (for instance, Belon, in his *Book of Birds*, 1555) between the bones of man and the bones of birds, constituted a problem, rather than an explanation. And one possible way of solving such a problem is by establishing a genetic relationship, or kinship, between the similar phenomena. In that way the Evolutionary Method finds a natural place among the methods of the comparative sciences.

CHAPTER V

THE SIMPLER INDUCTIVE METHODS

§ 1. *Classification and Law.*

The scientific search for general truths is satisfied in some measure by the discovery of natural classes through the Method of Classification, and by ascertaining evolutionary sequences with the aid of the Comparative Method. In the former case, we discover certain uniformities of co-existence among the groups of essential characteristics of the several natural classes. In the latter case, we discover certain uniformities of sequence among various complex phenomena, which follow one another as successive phases or stages of an evolutionary process. On the strength of our knowledge of a uniformity of co-existence among the attributes of a class, it is possible to infer from the presence of some class-characteristics in an object (e.g. the frontal horns and the hoofs of a quadruped) also the presence of certain other characteristics (e.g. the possession of a ruminant stomach, and graminivorous habits [1]); and on the strength of our knowledge of a uniformity of sequence among the stages of the

[1] There is a story that the devil once came to Cuvier, the famous zoologist, and threatened to eat him up. The zoologist looked the devil up and down, and replied: "You can't! You have horns and hoofs! Go and eat grass; you can't eat me!"

evolution of certain phenomena, one might anticipate the coming of a subsequent stage from the observation of an earlier one, or imaginatively interpolate something between two known stages.

But such uniformities as the foregoing are for the most part only empirical, and not altogether satisfactory. Science looks as far as possible for what can be more or less adequately proved. Even the relative values of different classifications will be assessed according to their usefulness for real inductions. Now all the proofs of general truths take one of two forms. One of them is the type with which we are familiar from geometry, where it can be shown by sheer intuition, or by deductive reasoning from intuition, that certain attributes must be present where certain other attributes are present; but the fundamental uniformities of natural science cannot be established in that way. The other method of proving uniformities is by ascertaining either the direct or indirect causal connection between the terms of the uniform relationship. In both forms of proof we try to establish relations between conditions and consequents; only in Mathematics (also in Logic) we are concerned with *rational* conditions (or reasons) and consequents, while in natural science we are concerned with *physical* conditions and results (in psychology with psychical conditions and results which are intimately connected with physical or physiological conditions and results).

The world, as we see it, is a vast complex of incessantly changing things, which the human mind

endeavours to grasp by mentally, and sometimes also physically, analysing into simpler constituents, and ascertaining the laws or regularities of their connections, or their correlations, if there be such. The facts themselves do not manifest their intimate relations with one another. We can only solve the riddle, if at all, by surmising what the relations may be. Such surmises are only fruitful when they have been preceded by close observation of the facts and are followed up by a still more searching observation and (where possible) experimentation.

§ 2. *The Five Canons or Methods of Induction.*

The kinds of observations by which the man of science is led to surmise a real connection between certain facts, and the kinds of observation by which he then proceeds to test his surmise, or hypothesis, are often very similar. Their general character has been formulated in the five so-called Canons of Induction. These are not the only methods of ascertaining laws, or uniformities, or regularities among the phenomena of Nature. We have already described two other methods of doing so, and yet other methods will be explained in due course. But the five canons or methods of induction are important all the same.

The principle underlying these canons is this. If, other things remaining essentially the same, a certain factor or circumstance cannot be omitted, or quantitatively changed, without changing a certain phenomenon, then that factor, or circumstance, is a condition of that phenomenon, or, in

other words, is intimately connected with it. Assuming, as we generally do, that things and events are not merely a matter of chance, but are the results of operative conditions, we examine instances of the phenomenon, in which we are interested, under sufficiently varied circumstances to enable us to detect what it is that cannot be removed or altered without removing or altering the phenomenon in question. Not that the presence of an element of chance in the universe is to be ruled out *ab initio*. We shall return to this point in due course. But it is order and regularity that have helped man most in his struggle for existence. It is order also that satisfies most the rational tendency and the æsthetic sense of man. So we naturally look for order first, and only reluctantly relinquish our search for it when we are baffled in our quest.

Now the process of ascertaining what is indispensable to a certain phenomenon may assume one of two forms, a direct form and an indirect one. In the direct form it is shown by observation or experiment that (*a*) the elimination, or (*b*) the quantitative variation of a certain factor or antecedent is followed by (*a*) the elimination, or (*b*) the quantitative variation of the phenomenon under investigation, although all other relevant factors have remained the same. In the indirect form of the inductive process it is shown that, so long as a certain antecedent remains operative, no change in any of the other relevant circumstances makes any material difference to the phenomenon under investigation, which must, therefore, be intimately

connected with the constant antecedent. The first type of the direct form is known as the Method of Difference, the second type of the same form is called the Method of Concomitant Variations. The indirect form is known as the Method of Agreement. Of the two remaining Canons or Methods, one is known as the Method of Residues, and is really a slight modification of the Method of Difference, while the other is known as the Joint Method of Agreement and Difference (also as the Method of Exclusion, or as the Double Method of Agreement), and is a kind of approximation to the Method of Difference, secured by supplementing the Method of Agreement in certain directions.

The several inductive methods have different degrees of cogency. The Methods of Difference and of Concomitant Variations are the most conclusive. But even these methods cannot always be applied rigorously. When, as sometimes happens, the phenomena under investigation are not sufficiently under the control of the investigator, he may not be able to secure the precise kinds of instances required for the strict application of these methods. But, in science, as in life generally, if one cannot command the best means, one tries the next best, and so on. In such cases, one usually endeavours to strengthen the result of the less strict application of one inductive method by the application of some of the other inductive methods as well, sometimes even by resorting to deductive reasoning from the nature of the case. The separate exposition of the several inductive methods must not be taken to imply that

each of them is usually, or should be, employed alone. They are frequently employed in conjunction; and in their less cogent forms it is not always easy to distinguish one from another, say, the Method of Difference from that of Concomitant Variations or from the Joint Method.

§ 3. *The Method of Difference.*

If two sets of circumstances are alike in all relevant respects except that in one of them (called the Positive Instance) a certain antecedent is present and also a certain consequent, while in the other (called the Negative Instance) both are absent, then that antecedent and that consequent are related as condition and consequent, that is to say, that consequent will always follow that condition. Symbolically, if antecedents a,b,c,d are followed by consequents w,x,y,z, while when d is absent the antecedents a,b,c are not followed by z, then d is a condition of z.

$$\underbrace{a\ b\ c\ d\ \ldots}_{w\ x\ y\ z\ \ldots}{}^{[1]}\ ; \qquad \underbrace{a\ b\ c\ \ldots}_{w\ x\ y\ \ldots}\ ; \qquad \text{therefore } \begin{matrix} d \\ | \\ z \end{matrix}$$

For example, suppose a piece of litmus paper when dipped into acid turns red at once, while another exactly similar piece of litmus paper not dipped into acid (but dipped, say, into water or some other liquid) does not turn red, then the acid is a condition

[1] The dots after the symbols (throughout this Chapter) are intended to make clear that there are other antecedents, and other consequents also present, which, however, are not considered relevant to the problem.

of its turning red. Again, suppose a surface exposed to the air, and having the same temperature as the air, is dry, while as soon as the temperature of the surface falls below that of the air, then, although the remaining circumstances remain the same, condensation of moisture takes place on that surface, in that case the lower temperature will be regarded as a condition of the condensation. Similarly, if a healthy animal is inoculated with the blood of another animal suffering from anthrax (splenic fever) and contracts the disease, then the inoculation will be regarded as a condition of the infection. Or, again, if a freshwater crayfish, having its antennules (small feelers) intact, retreats from strong odours, while another, bereft of them, does not react to strong odours at all, then it may be inferred that the antennules are the seat of the organ of smell.

To secure the requirement that only one relevant circumstance should distinguish the two cases compared, it is often necessary to use technical aids. In some cases indeed one could not determine at all the actual influence of one of the factors without technical aid. For example, the weight of air, the influence of air on moving bodies, its function in breathing, burning, and rusting, could not be ascertained at all without the aid of an air-pump. What the air-pump does, however, is to procure for us the requisite kind of instances about which we can reason satisfactorily, on the lines of one or other of these Inductive Methods. In simpler cases we can secure the right instances, to

reason about on similar lines, without the aid of technical devices.

The Method of Difference assumes various forms according to circumstances. Sometimes the two instances compared are really two successive states of the same set of circumstances, to which something is added to obtain the Positive Instance, or from which something is withdrawn to obtain the Negative Instance. On the other hand, the two instances are frequently separate and distinct instances, though similar in all essentials but one. And sometimes, again, the instances compared are not single instances, but are groups of instances, each group being treated more or less as a single instance. The following example may illustrate the group-form of the Method of Difference. When Pasteur tested the efficacy of preventive vaccination against anthrax, which he thought he had discovered, he first vaccinated twenty-five sheep with a mild preparation of the serum, and, when they had recovered, he vaccinated them again, and also twenty-five other sheep, which had not been vaccinated before, with a strong preparation of the serum. The twenty-five sheep which had undergone the preparatory vaccination survived, while the others all perished. This showed that the preparatory vaccination had acted as a protection. The two instances, in this case, were not single sheep but two groups of twenty-five sheep in each, and the result was all the more conclusive, for it is easier to secure essential similarity between two groups, as groups, than between two individuals.

In this, as in every other, form of the Method of Difference, it is most important that the two instances should be as like as possible in all essentials except the difference under investigation. The neglect of this condition (sometimes called the fallacy of *non ceteris paribus*) easily leads to the mistaken attribution of a result to the wrong circumstance or antecedent (the fallacy of *cum* or *post hoc ergo propter hoc*). The following example may serve as a warning. In a certain hospital in Dublin it was observed that there was a higher rate of mortality among the patients lying in the wards on the ground floor than among those lying in the wards on the upper floors. It was, accordingly, concluded that the ground floor was less healthy than the upper floors. Subsequently, however, it was ascertained that the hospital porter had been in the habit of placing in the wards on the ground floor all patients who were too ill to walk, while those who could walk were taken to the wards on the upper floors.

One of the most important precautions which must be taken in connection with the application of the Method of Difference is this. The introduction of the new factor ("d" in the above symbols) must be so managed that it does not alter in any way in the process of being introduced. Delay is sometimes fatal in this respect. For example, in the course of some experiments in connection with anthrax, two French professors obtained some blood from an animal that had suffered from the disease, and they injected some rabbits with it.

The rabbits died rapidly, but did not develop the parasites of anthrax. The French professors thereupon thought that they had disproved the view of Pasteur that anthrax could be communicated in that way. It turned out, however, that an interval of about twenty-four hours had elapsed between the drawing of the blood from the diseased animal and its injection in the healthy rabbits, and in the meantime the blood had putrified, so that when injected it set up a form of blood poisoning, which killed the rabbits so quickly that the anthrax parasites had no time to multiply sufficiently to manifest themselves.

§ 4. *The Method of Concomitant Variations.*

If an antecedent and a consequent vary concomitantly although no other relevant circumstance has changed, then that antecedent is a condition of that consequent. Symbolically :—

$$\underbrace{a\ b\ c\ d_1 \ldots}_{w\ x\ y\ z_1 \ldots}\ ; \quad \underbrace{a\ b\ c\ d_2 \ldots}_{w\ x\ y\ z_2 \ldots}\ ; \quad \underbrace{a\ b\ c\ d_3 \ldots}_{w\ x\ y\ z_3 \ldots}\ ;$$

[or briefly, $z = f\ (d)$, i.e. z is a function of d]

therefore $\dfrac{d}{z}\cdot$

The Method of Concomitant Variations is closely related to the Method of Difference, and can easily be expressed in a symbolic form very similar to that of the latter. Let the quantitatively variable antecedent in two instances be represented (as

above) by d_1 and d_2 respectively, and the quantitatively variable consequent by z_1 and z_2 respectively, then $d_1 = d_2 + (d_1 - d_2)$, and similarly $z_1 = z_2 + (z_1 - z_2)$. The two instances can accordingly be symbolized, one as a positive instance, the other as a negative instance, thus :—

$$\frac{a\ b\ c\ d_2 + (d_1 - d_2) \ldots}{w\ x\ y\ z_2 + (z_1 - z_2) \ldots}; \quad \frac{a\ b\ c\ d_2 \ldots}{w\ x\ y\ z_2 \ldots};$$

therefore $\dfrac{(d_1 - d_2)}{(z_1 - z_2)}$, or the quantitative difference in the antecedent d is connected with the quantitative difference in the consequent z. (Compare the symbolic form in § 3 above.)

The two methods are, of course, not quite the same, even so. In the Method of Difference it is shown that the *presence* of a certain antecedent is connected with the *presence* of a certain consequent; in the Method of Concomitant Variations it is shown that a certain *quantitative difference in a given antecedent* is connected with a certain *quantitative difference in the consequent*. But the difference between the two methods is not always important.

The tendency of modern science to be quantitatively exact has given special importance to the Method of Concomitant Variations. Over and above its special function to show that there is a connection between certain phenomena in the study of which the other inductive methods are not so helpful, the Method of Concomitant Variations is also applied in all possible cases just to ascertain the quantitative correlation between phenomena,

even though their connection is not under consideration—either because the connection as such has already been established, or because it is not believed that there is any direct connection between the phenomena in question. This use of the method is usually described as *quantitative* induction, in contrast with *qualitative* induction, which merely seeks to ascertain (by means of any of the inductive methods) whether there is any connection at all between certain phenomena, without determining their precise quantitative co-variation. As examples of quantitative induction we may refer to the experiments by which the coefficient of expansion of various substances is determined, or the coefficient of absorption and emission of heat, or the relation between the amount of the radiation and the temperature of bodies, or the mechanical equivalent of heat, or the refractive index of a transparent medium, or the relation between the electric current passing through a conductor and the electric pressure between its ends, and so on. Most, or all, statistical correlations also belong here.

The Concomitant Variation may be direct or inverse. It is said to be *direct* when the antecedent and the consequent increase together, and diminish together. It is said to be *inverse* when one of them diminishes as the other increases. Thus the temperature and the volume of a gas (the pressure remaining constant) are an instance of direct concomitant variation, while the pressure and the volume of a gas (the temperature remaining constant) are an example of inverse concomitant

variation. Some phenomena exhibit direct and inverse variations at different stages. For example, the temperature and the volume of water show direct concomitant variation between 4° C. and 100° C., but inverse concomitant variation between 0° C. and 4° C. Moreover, the same consequent may be connected with two conditions and vary directly with one of them, and inversely with the other. For instance, the gravitation between bodies varies directly with their masses and inversely with their distances from one another.

Again, as the last example suggests, the concomitant variation (whether direct or inverse) may be simply proportionate, or highly complex. For instance, gravitation varies in simple proportion to the multiple of the masses of the bodies, but it varies inversely with the square of their distances. In some cases the concomitant variation requires a very complicated formula for its expression, and in some instances the concomitant variations are so complex that the correct formulæ for them have not yet been discovered.

The Method of Concomitant Variations is applicable in some cases in which the Method of Difference is impracticable, namely, in all cases in which the conditions studied can be varied in quantity, or intensity, but cannot be eliminated altogether. This applies to heat and gravitation, neither of which can be completely eliminated from material bodies. It is also true of the friction of moving bodies, since no case is known of frictionless motion. Since the amount of friction can be varied

enormously, and it is found that the period of motion of a body varies inversely with the amount of friction which it encounters, we conclude that, in the limiting case, if friction could be eliminated altogether, then a body once in motion would continue to move indefinitely. Thus the first law of motion really rests on the Method of Concomitant Variations.

The following is an interesting instance of the application of the Method of Concomitant Variations. During the cholera epidemics in London in 1849 and in 1854 about a fifth of the population of London was supplied with water by the Lambeth Water Company and the Southwark Water Company. In 1849 both companies obtained their water from the same part of the Thames, so that the water supplied by both companies was equally polluted. The number of deaths from cholera per 10,000 of the population in the area served by the Lambeth Company was 125, and in the area served by the Southwark Company it was 118, that is, nearly the same. In 1854 the Lambeth Company drew its water much higher up the river, where the water was far less polluted, while the Southwark Company retained its old intake. The mortality rate from cholera per 10,000 of the population in the area served by the Lambeth Company fell to 37, while in the area served by the Southwark Company it rose to 130. Evidently the contaminated drinking-water was one of the conditions of the cholera mortality. But polluted drinking-water is something complex calling for further analysis, and

subsequent investigation revealed what specific form of water-contamination was connected with cholera. It is not at all uncommon in the history of science that the condition of some phenomenon is first tracked to a complex antecedent from which the real condition is then fetched out, as it were, from its hiding-place by a closer scrutiny, that is, by more careful analysis.

In the application of this, or any other, inductive method, inadequate analysis of circumstances may easily mislead one to attribute a result to the wrong antecedent, as the following case may illustrate. Dr. W. Farr, an early investigator of the epidemics of cholera referred to in the preceding paragraph, discovered an inverse concomitant variation between the number of deaths from cholera per 10,000 inhabitants and the elevation of the district in which they resided. The following table gives his data :—

Elevation of District in feet.	*Cholera Deaths per* 10,000 *inhabitants.*
Under 20	102
20 – 40	65
40 – 60	34
60 – 80	27
80 – 100	22
100 – 120	17
340 – 360	7

He concluded, accordingly, that "the elevation of the soil in London has a more constant relation with the mortality from cholera than any other known element." Now, Dr. Farr was on the right scent—he was, so to say, getting near the hiding-place of

one of nature's secrets. The really important factor, as explained in the foregoing account of the matter, was a specific pollution of the drinking-water. But at that time London was, to a large extent, supplied with water from surface-wells. Now, wells at or about the river level had their waters more polluted than those at a higher level. Moreover, the lower-lying districts of London were more densely populated, and consequently more exposed to infection.

§ 5. *The Method of Agreement.*

If several instances of the occurrence of a phenomenon have one relevant antecedent in common, then that common antecedent is a condition of that phenomenon. Symbolically:—

$$\underbrace{a\ b\ c\ d\ \ldots}_{w\ x\ y\ z\ \ldots};\quad \underbrace{b\ d\ f\ g\ \ldots}_{x\ z\ s\ t\ \ldots};\quad \underbrace{d\ f\ k\ l\ \ldots}_{z\ s\ p\ r\ \ldots};\ \text{therefore}\ \begin{matrix} d \\ | \\ z \end{matrix}.$$

If dew is deposited on a number of surfaces which are different in all relevant respects except that their temperature is below that of the surrounding atmosphere, then the lower temperature is a condition of the deposit of dew. Take another example. Brewster took impressions from a piece of mother-of-pearl in a cement of resin and beeswax, in balsam, in fusible metal, in lead, in gumarabic, in isinglass, etc. In all cases the same iridescent colour appeared. But the only character which these substances had in common was the form of the surface produced by the impression of the piece of

mother-of-pearl. Hence that form of surface must be a condition of the iridescent colour.

No scientific, or other, method is fool-proof, and the Method of Agreement is perhaps least so. In the absence of an adequate analysis of circumstances, even in spite of the most scrupulous caution, mistakes are easily committed. One might conceivably argue that since different drugs, which had proved fatal, appeared to have nothing in common except water, or moisture, therefore water is poisonous. But the absurdity of this is too transparent. It is different when the common antecedent is something complex, and insufficiently analysed and understood at first. For example, at one time the presence of marshes was regarded as the common condition of epidemics of malaria. This was really the clue to the subsequent discovery that mosquitoes (which abound in marshy regions) are the carriers of malaria. For the rest, the chief dangers to which the Method of Agreement is exposed are: (*a*) that a relevant circumstance may be overlooked; (*b*) that consequents not precisely alike, except perhaps for practical purposes, may be treated as essentially similar, and assigned to a common circumstance, which is not the condition at all (for instance, the above example relating to drugs); (*c*) that the antecedent and consequent may both be the consequents of the same condition, or set of conditions, as is the case, for example, with the sequence of day and night, or the sequence of the phases of the moon, or the sequence of the seasons of the year—day is not a condition of night, or *vice versa*, but the whole

sequence of day and night is conditioned by the rotation of the earth, and similarly with the other sequences just mentioned.

§ 6. *The Method of Residues.*

If part of a complex result can be accounted for by certain antecedents which are known to have been operative, and the nature of whose consequents is already known as the result of previous investigations, then the residue of the complex result must be due to the remaining operative condition or conditions. Symbolically :—

$$\underbrace{a\ b\ c\ d\ \ldots}_{w\ x\ y\ z\ \ldots}; \quad \left[\underbrace{a\ b\ c\ \ldots}_{w\ x\ y\ \ldots}\right]; \quad \text{therefore} \quad \begin{matrix} d \\ \vert \\ z \end{matrix}.$$

Sometimes such other conditions are already known to be present and it is only a matter of determining their precise effects. More often their presence is not even suspected until the residual phenomena compel one to search for them. For example, the weight of coal in a truck may be determined as a residual phenomenon if one knows the net weight of the empty truck and deducts it from the gross weight of the loaded truck. Or the resistance of the air on the trajectory of a bullet may be determined by observing the deviation of its actual trajectory from that which could be accounted for by the value and direction of the propelling force and the force of gravitation. In both these examples the presence of the residual condition is known, only its weight, or influence, has to be determined. In other cases, and these are

the most important cases, it is different. Thus, for example, the existence of argon was not suspected until the residual density of atmospheric nitrogen (that is, nitrogen obtained from the atmosphere by removing impurities, moisture, oxygen, etc.) in comparison with chemical nitrogen (that is, nitrogen prepared from nitrous oxide, or nitric oxide, etc.) was observed. Similarly the existence of the planet Neptune was not thought of until the residual deviation in the orbit of Uranus made astronomers look for it. Nor was the velocity of light known, or surmised, until attention was directed to it by the (residual) difference in the observed periods between successive eclipses of Jupiter's satellites, according as the earth in its orbital motion was moving towards or away from Jupiter.

§ 7. *The Joint Method of Agreement and Difference.*

If a group of several instances in which a phenomenon occurs have nothing relevant in common except a certain antecedent, while another group of similar instances in which the phenomenon does not occur have nothing relevant in common except the absence of that antecedent, then that antecedent is a condition of that phenomenon. Symbolically :—

Positive group : $\underbrace{a\ b\ c\ d\ \ldots}_{w\ x\ y\ z\ \ldots}$; $\underbrace{b\ d\ f\ g\ \ldots}_{x\ z\ s\ t\ \ldots}$; $\underbrace{d\ f\ k\ l\ \ldots}_{z\ s\ p\ r\ \ldots}$;

Negative group : $\underbrace{b\ c\ f\ \ldots}_{x\ y\ s\ \ldots}$; $\underbrace{b\ g\ k\ \ldots}_{x\ t\ p\ \ldots}$; $\underbrace{c\ l\ a\ \ldots}_{y\ r\ w\ \ldots}$;

therefore $\begin{matrix} d \\ | \\ z \end{matrix}$.

For example, Lord Avebury found that various insects differing in many respects but all having compound eyes can see at a distance, whereas other insects having ocelli, but not compound eyes, cannot see at a distance. He concluded, accordingly, that compound eyes are a condition of distant vision. Another example, Darwin observed that many plots of land containing all of them plenty of earth-worms, although otherwise very different in character, became covered increasingly with vegetable mould, whereas, on the other hand, many plots of land not essentially unlike the former plots as a whole, but deficient in earth-worms, did not get covered with vegetable mould. He therefore concluded that the vegetable mould is due to the agency of earth-worms.

It should be observed that the positive and negative instances which are sufficient for the application of the Joint Method are not such as would make it possible to employ the Method of Difference. There may be no negative instance sufficiently similar to any of the positive instances to meet the requirements of the latter method. All that can be said is that if the whole group of positive instances is regarded as though it were one positive instance, and if the whole group of negative instances is regarded as though it constituted one negative instance, then the Joint Method appears as an approximation to the group-form of the Method of Difference.

CHAPTER VI

THE STATISTICAL METHOD

§ 1. *The Method of Simple Enumeration and Exact Enumeration.*

The Inductive Methods described in the preceding chapter can only be applied fruitfully where the facts investigated can be analysed adequately, and examined under sufficiently varied conditions. This is equally true of the more advanced methods which will be considered later. Now these requirements cannot always be satisfied. The facts investigated may be too complicated for adequate analysis, and may not be observable under sufficiently varied circumstances. In such cases it is impossible to ascertain with confidence the thread of connection between conditions and consequents by means of the above-mentioned inductive methods. The kind of facts here considered may be indicated by reference to meteorological, economic, social, medical and various biological problems.

Popular thought, impelled by practical needs and by its proverbial incapacity to suspend judgment, resorts in such cases to what is known as the Method of Simple Enumeration. That is to say, the concurrence or sequence of certain attributes, or circumstances, or events is noted, and, if a

concurrence or sequence is observed a considerable number of times, it will be assumed that the facts or events in question are connected with one another as condition and consequent, or, to use a more popular mode of expression, as cause and effect. Popularly this method, such as it is, is employed even in cases where more satisfactory methods might be applied. No attempt is usually made to observe sufficiently varied instances of concurrence or of sequence (as is required, for example, for the application of the Method of Agreement), nor, as a rule, is attention paid to exceptions. It is only called a method by courtesy. It is a loose habit of mind rather than a scientific method. Its uselessness is attested by many popular fallacies and superstitions. A full moon is commonly believed to bring fine weather, because the two have often been observed together; it is, of course, on fine nights only that the average person takes cognisance of the full moon, and he does not think of ascertaining if the weather is not also bad sometimes when there is a full moon. Similarly, many otherwise intelligent people still stand in awe of the number 13, which private hosts, as well as hotel managers, carefully avoid.

Now, the scientific method, which is usually employed in such complex cases as are not amenable to the other inductive methods, is the Statistical Method. We have already made our first, though slight, acquaintance with this method as an auxiliary to the Method of Classification, namely, as an aid to adequate description in certain types of

cases (see Chapter III, § 2). It may be used similarly as a descriptive auxiliary to the Comparative Method. Indeed, much of the material examined in connection with problems of biological evolution has been, and is being, classified and tabulated in accordance with Statistical Methods. But the most important use of the Statistical Method is as an independent scientific method for ascertaining connections, or laws, and regularities, Like the Method of Simple Enumeration, it notes concurrences; but, unlike it, it is careful also to note and record exceptions, to make observations over as large and varied a field as possible, and then to proceed cautiously to interpret the whole of the observations made and recorded.

§ 2. *Statistical Processes.*

Scientific investigation is always concerned with the discovery of the relationship between two or more attributes or variables. By an "attribute" is here meant anything the bare presence or absence of which can be noted and counted, but which is not otherwise measurable; by a "variable" is here meant anything that has a magnitude that is measurable, and which may be present in different magnitudes. The Statistical Method seeks to discover whatever regularity might subsist between two or more attributes, or two or more variables. Now the concurrence of two or more attributes, or the correspondence of two or more variables, may be merely a chance coincidence, or it may be the result of some direct or indirect connection between

them. By observing only one instance, or a small number of instances, of the concurrence or the correspondence, and that under conditions beyond our control, and under circumstances not adequately known, it may be impossible to distinguish between a casual and a causal concurrence or correspondence. But the observation of a large number of instances taken from a wide range, and an exact enumeration of both positive and negative cases, and of variations between series of cases, may enable us to draw a highly probable conclusion about the connection between the phenomena in question. Such procedure, based on exact enumeration, is of the essence of the Statistical Method.

The processes or stages involved in a complete application of the Statistical Method may be described as follows :—

(*a*) *Collection of Material.* The facts or data under investigation, or, more usually, adequate samples of them, are observed, counted or measured, and described in a way relevant to the problem in hand. The measurements and descriptions must obviously be sufficiently accurate, if they are to be of value, but the degree of precision required will vary with different investigations. To avoid one-sidedness, it is desirable that the facts should be collected from as wide and varied a field as possible. In some statistical inquiries the data are collected by means of questionnaires, sometimes of an official character (like the census, or various trade schedules), sometimes of a private character (like those sent out by the late Sir Francis Galton, or by Professor Karl

Pearson, and others). In such cases it is necessary, though not always easy, so to frame the questions as to reduce to a minimum the danger of obtaining misleading answers.

(*b*) *Classification, Tabulation and Correlation of Material.* The facts or data are then classified and tabulated with respect to certain attributes, or variables, in which the investigator is interested. Examples of simple Classification and Tabulation have already been given in Chapter III, § 2, and may be referred to now. For merely descriptive purposes, such simple tables dealing only with one attribute, or one variable, may be useful; but for further investigation we require tables giving two or more attributes (such as, say, the colour of eyes and of hair), or two or more variables (such as the supply and the price of wheat, or of some other commodity, over a period of years, or the temperature of different geographical areas and their latitude, or the length and breadth of leaves). The table on page 77 may serve as an example of a simple type of correlation or contingency table, as such a twofold (or multiple) table is called.

(*c*) *Summarizing the Tables.* The data classified and tabulated are often very numerous and complicated. It may be difficult to see the wood for the trees. A concise summary of the results by the aid of averages, coefficients of association and of correlation is, therefore, helpful or even necessary. Graphs and other diagrams are also a useful aid for bringing the results home. It is this stage especially that calls for a knowledge of mathematics

and of statistical technique. (See Chapter III, § 2, above.)

(*d*) *Critical Interpretation.* As a result of the foregoing processes it may next be possible to state the extent and character of the relation between two or more attributes or variables that have been investigated. There may be no association or correlation [1] between them at all. That is to say,

Eye colour.	Hair Colour.		Total.
	Fair.	Dark.	
Light ..	2,714	3,129	5,843
Brown ..	115	726	841
Total ..	2,829	3,855	6,684

Proportion of light-eyed with fair hair $\frac{2,714}{5,843} = 46$ per cent.

Proportion of brown-eyed with fair hair $\frac{115}{841} = 14$ per cent.

the occurrence of the one attribute, or the value of the one variable, may show no regular correspondence with that of the other. They may occur together or correspond sometimes, but that may be a mere coincidence. On the other hand, the

[1] The term "association" is usually employed to express the relation between two or more attributes, as defined above. The term "correlation" is, on the other hand, usually restricted to express relation between variables. But the distinction is not always adhered to.

presence or absence of the two attributes, or the variations of the two variables, may show a regular correspondence. In that case they may be intimately connected; their concurrence or correlation may be something more than a mere coincidence. This may be the case also even when the association of the two attributes, or the correlation of the two variables, is not complete but only partial. In the best cases we are thus led to the discovery of a law, that is to say, a relation which we have reason to believe to be uniform. In somewhat less successful cases we may still be able to formulate a regularity of some kind.

The establishment of some law or regularity of connection may be said to be the natural end of the Statistical Method at its best. The word "end" is here used with conscious ambiguity. The Statistical Method, like all scientific methods, aims at the discovery of general truths, if possible. On the other hand, as soon as such general truths are discovered in any department of inquiry, the Statistical Method is apt to be superseded. There is no further interest in noting the frequency of the occurrences or facts in question, when their laws are already known. For example, there was a time in the history of Astronomy when records were kept of solar and lunar eclipses. It was on the strength of certain observed, but as yet unintelligible, cycles that the ancients already foretold eclipses with some accuracy. But since the laws of the occurrence of eclipses have been discovered there is no further need to keep statistical records of their occurrence.

Eclipses can be foretold with great accuracy and certainty.

Laws, however, are not so easily discovered, and the Statistical Method is not fool-proof. Great care is required for the correct interpretation of associations and correlations. Even a high degree of association of attributes, or of correlation of variables, may be no conclusive evidence of real connection. A few simple examples may make clear some of the types of rash interpretation. The fact that a high percentage of full-moon nights are fine is no evidence of real connection between full moon and fine weather. A comparison with other nights, when there is no full moon, shows that the percentage of fine nights when the moon is not full is just as high. Again, the fact that the mortality of babies who use comforters is six times as great as that among children who go without comforters, is, according to Professor Pearson, also no evidence of a connection between the use of comforters and infant mortality. The higher mortality may be due to hereditary weakness of the children who use comforters; the very use of comforters may only be a symptom of the children's weakness and consequent irritableness. Apparently, with a little ingenuity it is possible to correlate the spread of cancer with the increased importation of apples, and the expenditure on the Navy with the growing consumption of bananas, at least so Professor Pearson suggests. If so, it is obvious that mere statistical technique is no adequate substitute for common sense, and scientific insight.

§ 3. *Kinds of Association and Correlation.*

In its most fruitful applications the results of the Statistical Method are in some ways very like those of the Method of Agreement and the Method of Concomitant Variations, although the processes are different in some respects. In any case, the associations or correlations established by the Statistical Method, and the concomitant variations shown by the Method of Concomitant Variations, exhibit analogous types. There are Positive and Negative Associations and Correlations, just as there are Direct and Inverse Concomitant Variations; and there are Simple and Complex Associations and Correlations, just as there are Simple and Complex Concomitant Variations. (See Chapter V, § 4, above.) One important difference is noteworthy: whereas the Method of Concomitant Variations, like the other inductive methods, is only concerned with the discovery of uniform relations, or laws, in the stricter sense, the Statistical Method is concerned with the discovery of partial associations and correlations, as well as with the discovery of complete associations and correlations. Moreover, for purposes of merely qualitative induction (see Chapter V, § 4), the Method of Concomitant Variations can be employed in cases in which quantitative variations can be observed but cannot be measured with any accuracy; the Statistical Method, on the other hand, is only applicable to phenomena which, directly or indirectly, can be measured with accuracy.

The association between two attributes is said to be positive if the presence of one is accompanied by the presence of the other; it is said to be negative if when one of them is present the other is absent. Similarly, the correlation of two variables is said to be positive if an increasing value of one of them corresponds to an increasing value of the other. It is said to be negative when an increasing value of the one corresponds to a diminishing value of the other. Negative association or correlation must be distinguished from the absence of association or of correlation. There is an absence of association between two attributes when the presence or absence of one of them corresponds in no way to the presence or absence of the other, their concurrence or otherwise being a matter of chance. Similarly, the absence of correlation between two variables means the absence of any kind of correspondence between their values. In contrast with such absence of correlation and association, negative correlation and negative association are real correlation and real association, just as, in the case of concomitant variation, inverse concomitant variation is also a real form of concomitant variation, and quite different from an absence of concomitant variation.

Again, the association between two attributes, or the correlation between variables, may be complete, so that we can express it in the form of a general truth or law, such as "all cases of A are cases of B," or "$A = c(B)$," where c stands for some ascertained constant. Complete association or

correlation is sometimes expressed by 1 and sometimes (in the U.S.A.) by 100; and positive and negative association or correlation are expressed by + and − respectively. So that + 1 (or + 100) would express complete positive association or correlation, and − 1 (or − 100) would express complete negative association or correlation. Absence of association or correlation is expressed by 0. Partial association or correlation will be expressed by numbers intermediate between 0 and 1 (or 100). Such numbers are known as coefficients of association (sometimes symbolized by Q), or coefficients of correlation (usually symbolized by r). The coefficient + ·8 (or + 80) would be considered to express a high degree of positive connection. It is in fact the coefficient of correlation between the stature of man and his cubit (that is the length of the arm from the elbow to the tip of the middle finger), also between the cubit and the height of the knee.

§ 4. *The Value of Descriptive Statistics.*

The results of the application of Statistical Methods are often valuable, even when they do not lead to explanation, or when they do not establish any connection between the phenomena, or even disprove an alleged connection. Through exact descriptions, by means of accurate counting and measuring, classifying and tabulating, the phenomena under investigation assume an orderliness which renders them easier to grasp, and such orderliness clearly paves the way for future dis-

coveries and explanations. Many of the results of the application of Statistical Methods are also very useful to the individual and to society. This is evident from the fact that the whole business of insurance rests on statistical calculations and processes. Life contingencies, or mortality tables, are an excellent help to insurance companies, even if they throw no light on the complex conditions which determine life and death. The statistics of births, marriages, imports, exports, etc., furnish a certain amount of guidance in practical affairs, besides preparing the ground for further sociological and economic research. Even the knowledge of the number of suicides per thousand of the population in a given country for a given period of years may throw some light on social and economic conditions. When the rate of such an occurrence shows comparative regularity for a period of years, that may be taken as an indication that certain social or other conditions have not changed much during that period. But that of itself is no evidence that the relevant conditions, whatever they may be, cannot or will not change; and, with any change in these conditions that may come, the rate of occurrence in question is also liable to change. Within certain limits, or with reference to a short future period, it may be safe enough to rely upon the regularity of past rates, so long as there is no evidence of any striking changes in the relevant conditions. But it is an unscientific extravagance to raise any such observed regularity of the past to the rank of an invariable and inviolable law. Yet that is what

Buckle did, when he maintained that in a given state of society 250 persons *must* in the course of each year put an end to their lives, that so and so many letters *must* miscarry, and so on. It happens only too frequently that people fail to realize the nature of their assumptions and inferences, or, indeed, fail to realize that they are making assumptions and inferences at all, when they treat the chronicle of the past as an almanac for the future.

While there can be no two opinions about the helpfulness of statistical technique for the orderly description and preparation of material for further scientific investigation, there is no such confident unanimity about the self-sufficiency of statistics as an independent method of scientific interpretation. This is intelligible. The correct interpretation of phenomena requires, above everything else, a thorough familiarity with the phenomena themselves. An expert chemist will achieve far more with inferior apparatus than an amateur can hope to achieve with the best apparatus. Similarly, an expert biologist or psychologist is likely to interpret his facts more accurately, even if he is not an expert in statistical technique, than a statistical expert who is an amateur biologist or psychologist. By setting up statistics, not only as an independent method, but as an independent science, a certain amount of encouragement may be given unintentionally to the conceit that anything which can somehow be counted and measured is grist for the statistical mill, and can be manipulated and

interpreted adequately by any statistician. But statisticians themselves, at least when commenting on the work of their colleagues, have had frequent occasion to point out the inadequacy of statistical technique alone to the complete solution of scientific problems.

CHAPTER VII

THE DEDUCTIVE-INDUCTIVE METHOD

§ 1. *The Combination of Deduction and Induction.*

In science, as in every kind of study, knowledge already acquired facilitates the acquisition of further knowledge. This was illustrated, to some extent, in connection with the Method of Residues (Chapter V, § 6). Considerable progress in the development of science may be rendered possible by combining the simpler inductive methods with deductive reasoning, either of them being used to confirm or to extend the knowledge obtained or obtainable by the other. The combination of deduction with induction has been named, by John Stuart Mill, the "Deductive Method"; but as this is rather liable to be confused with mere deduction, which is only one constituent of the combined method, it may be better to describe it as the Deductive-Inductive Method. As applied to the study of natural phenomena, Mill distinguishes two principal forms of the Deductive-Inductive Method, namely: (*a*) that in which the deduction precedes the induction, and (*b*) that in which the induction precedes the deduction. The former he calls the "Physical Method"; the latter he calls the "Historical Method." The distinction is of no fundamental importance, and

the names are very inappropriate. Both forms of the Deductive-Inductive Method are employed in Physics, and in other sciences. More important than the order in which the two parts of the method are applied, is the nature of the circumstances leading to the application of this whole method. Briefly, there are three kinds of occasions on which the Deductive-Inductive Method is employed: (1) When an hypothesis cannot be put to the test directly, but only indirectly; (2) when the attempt is made to systematize already accepted inductions or laws under more comprehensive laws or theories; (3) when, owing to the difficulties of the problem, or to the lack of sufficient and suitable instances of the phenomena studied, deduction and induction are employed by way of mutual support. Each of these types of cases may now be considered separately.

§ 2. *The Indirect Verification of Hypotheses.*

Sometimes an hypothesis, stating the possible nature of the connection between the phenomena studied, cannot be put to the test directly; only its consequences can be tested by observation or experiment in the light of already established knowledge (frequently including mathematical knowledge). The implications of the hypothesis are then deduced or calculated, by the aid of mathematical or other forms of deductive reasoning, until we arrive at such consequences as can be put to the test of observation or experiment. A simple example may help to make the difference clear. For this purpose, two hypotheses of Galilei

will serve. At the time of Galilei there was still current the Aristotelian hypothesis that the velocity of falling bodies varied with their weight. Galilei opposed this view and put forward the hypothesis that all bodies, no matter what their weight may be, fall through the same distance in approximately the same time (allowing for the resistance of the air). These conflicting hypotheses could be tested directly by dropping simultaneously bodies of different weights from the same height. Galilei did test them in this way, by dropping bodies of different weights from the leaning tower of Pisa, and thereby he disproved the Aristotelian hypothesis, while confirming his own. The method employed was that of Concomitant Variations, only the result was negative—the difference, or variation, in weight was not followed by any difference in velocity. Such simpler inductive methods, however, did not suffice when Galilei next undertook the task of ascertaining the real law of the velocity of falling bodies. After trying various hypotheses, there occurred to him, eventually, the hypothesis that a body starting from rest might fall with uniform acceleration, and that its velocity might vary with the time of the fall. But he could not think of any method of testing that hypothesis directly. By mathematical deduction, however, he concluded that if a body did fall in the way suggested by his hypothesis, then the distance through which it would fall should be proportionate to the square of the time of its fall. This consequence of the hypothesis could be tested directly, by comparing the actual distances traversed

by falling bodies during different times, or by comparing the times taken by the fall through different distances.

As another example of this use of the Deductive-Inductive Method we may refer to Newton's hypothesis that the orbital movement of the moon is determined by terrestrial gravitation. This hypothesis could not be tested directly by any of the simpler methods of induction alone. But, by deductive reasoning and calculation, Newton arrived at the conclusion that, if his hypothesis were true, then the moon should be deflected from its rectilinear path at the rate of approximately 16 feet per minute. Now, this consequence of the hypothesis could be tested, by observing and determining the orbit and period of the moon. Eventually the hypothesis was actually confirmed in that way, though, owing to a misconception about the length of the earth's radius (which was one of the data of his calculation), Newton abandoned his hypothesis for a long time. Still other instances of this use of the Deductive-Inductive Method are the Undulatory Theories of Light and of Sound.

§ 3. *The Systematization of Laws.*

The more developed sciences constantly endeavour to link up systematically such laws or regularities as they have already discovered. The greater the knowledge already possessed the more possible is it usually to interconnect it into a coherent system. Conversely, the more systematic the knowledge becomes, the deeper and more coherent does our

insight into the phenomena become. Speaking metaphorically, each isolated law reveals a continuous thread in the tangled fabric of Nature, but still only a single thread; when various laws are found to be systematically interrelated, then they reveal something of a whole pattern in the fabric of Nature. The usual way of establishing such systematization is by discovering some hypothesis from which certain laws, already obtained by previous inductions, but apparently standing in no relation to one another, can all be derived by deductive reasoning. The Law of Universal Gravitation is a familiar example of this kind of procedure. Kepler had discovered three most important laws of planetary motion, by induction from numerous astronomical observations made by Tycho Brahe and himself. The three laws were: (*a*) that the planets move in elliptic orbits having the sun for one of their foci; (*b*) that the velocity of a planet is such that an imaginary line (called the radius vector) joining the moving planet to the sun sweeps out equal areas in equal intervals of time; and (*c*) that the squares of the times which any two planets take to complete their revolutions round the sun are proportional to the cubes of their mean distances from the sun. Newton showed that these laws could all be deduced from the law that the planets (or, more generally still, all particles of matter) tend to move towards each other with a force varying directly as the product of their masses, and inversely as the square of the distances between them. In that way, laws (and phenomena) which

appeared to have nothing to do with one another were shown to be expressions (or manifestations) of the same systematizing principle.

Additional instances of this use of the Deductive-Inductive Method are furnished by the Kinetic Theory of Gases in its relation to Boyle's Law, to the Law of Charles, to the Law of Avogadro, etc., and by the Undulatory Theory of Light in relation to Snell's Law of Refraction, etc.

§ 4. *The Mutual Support of Deduction and Induction.*

In the study of certain kinds of highly complex phenomena which are beyond the control of the investigator, such as economic and other social phenomena, it is very unsafe to put much faith in the necessarily inadequate applications of the simpler inductive methods, and even more unsafe to trust purely deductive reasoning from a few elementary laws of human nature, etc. It is not safe to trust purely deductive reasoning, because there is the risk of overlooking all sorts of modifying or counteracting factors, so that the concrete result may be very different from that anticipated on deductive grounds. And, in the kind of cases here contemplated, it is also unsafe to put implicit trust in the inductions alone, because they are based on a comparatively few instances observed with difficulty under circumstances which are extremely complicated, not varied in the way required for the cogent application of the simpler methods of induction, and altogether beyond the control of the investigator. In such cases one does the best he can by the aid of

both deduction and induction, and if the two modes of procedure converge towards the same conclusion, then one's confidence in the result is naturally greater.

An interesting example of this use of the Deductive-Inductive Method is contained in Herbert Spencer's *Principles of Sociology*. The following is a very bald summary of the argument, giving just enough to bring out the character of its method, and no more. Spencer's aim is to prove a connection between industrialism and free institutions, or, conversely, between militarism and lack of freedom. The first part of the argument (*a*) is inductive, involving rough applications of the Methods of Agreement, Difierence, and the Joint Method. But, as the instances he can draw upon are rather few for such a complex problem, and not related to one another in precisely the way required for a rigorous application of the inductive methods, he endeavours to confirm his inductive conclusion by (*b*) independent deductive reasoning from the nature of the case, in the light of what is known of human nature.

(*a*) In Athens, where industry was regarded with comparative respect, there grew up an industrial organization which distinguished the Athenian society from adjacent societies, while it was also distinguished from them by the democratic institutions that simultaneously developed. Turning to later times, the relation between a social regime predominantly industrial and a less coercive form of rule than is usually found in societies which are predominantly militant is shown by the Hanse

towns, by the towns of the Low Countries out of which the Dutch Republic arose, by Norway, by the United States, by Britain, and the British colonies. Along with wars less frequent, and along with an accompanying growth of agriculture, manufacture, and commerce, beyond that of continental states more military in habit, there has gone in England a development of free institutions. As further implying that the two are related as cause and consequence, there may be noted the fact that the regions whence changes towards greater political liberty have come are the leading industrial regions, and that rural districts, less characterized by constant trading transactions, have retained longer the earlier (militant) type with its sentiments and ideas.

(*b*) The pervading traits in which the industrial type differs so widely from the militant type, originate in those relations of individuals implied by industrial activities, which are wholly unlike those implied by militant activities. All trading transactions are effected by free exchange. For some benefit which A's business enables him to give, B willingly yields up an equivalent benefit. This relation in which the mutual rendering of services is unforced and neither individual is subordinated, becomes the predominant relation throughout society in proportion as the industrial activities predominate. Daily determining the thoughts and sentiments, daily disciplining all in asserting their own claims while forcing them to recognize the correlative claims of others, it produces social units

whose mental structures and habits mould social arrangements into corresponding forms. There results a type of society characterized throughout by the same individual freedom which every commercial transaction implies. In the militant type, on the other hand, the nation is essentially an army, sometimes mobilized, at other times quiescent. And as the soldier's will is so suspended that he becomes a mere instrument of his officer's will, so the citizen of a militant regime is overruled by the government.

If inductive inference sometimes needs support from deduction, purely deductive reasoning (except perhaps in pure mathematics) stands in even greater need of inductive confirmation, especially in the case of complex phenomena. The history of science, especially of economic and social science, can point to many cases which should serve as a warning in this respect. Ricardo, for instance, arguing deductively, maintained that the continuous increase in population would necessitate the cultivation of less and less fertile soils; this would raise land rents, and increase the price of food. This deductive conclusion was falsified by improvements in agricultural methods, and by the cultivation of fertile soils at great distances, which was rendered possible by developments in transport. Similarly, some of the Malthusians, relying on inadequate deductions, arrived at rather pessimistic conclusions about the future of the working classes. It was argued that any improvement in the regularity and amount of their wages would only encourage them to have still

larger families, whose additional needs would continue to keep them at the poverty line. But the subsequent investigations of Charles Booth showed clearly that, as a matter of fact, the families of the working classes steadily diminished in numbers, and their standard of life steadily became higher, as their income improved in amount and in regularity. Deductive reasoning is, of course, sound enough as far as it goes; there is nothing intrinsically wrong with it. But, as already remarked, it is liable to be too abstract, in the sense of not taking into account all the factors involved. And it is just this that gives what little justification there is for the hackneyed dictum of men of practical affairs, namely, that this or that may be all right in *theory*, but will not work in *practice*. Except in purely hypothetical cases, what is true in theory is meant to be true in practice. But deductive theory is liable to overlook factors, whose actual influence is in no way diminished by being forgotten.

§ 5. *The Value of the Deductive-Inductive Method.*

Contrary to what might, at first, be expected, the indirect, more roundabout method of verifying hypotheses and establishing connections between phenomena, is scientifically more valuable than the direct method. The simpler methods of induction are frequently applicable where, as yet, there has been no great development of the science concerned. They are applicable where comparatively little is known as yet; but the Deductive-Inductive Method, especially in its more complicated forms, demands

a considerable amount of systematic knowledge, and so presumes a systematization on the part of the science in question, which, in its turn, it helps to systematize still further. For example, the ancient astronomers (long before Thales) had noticed that solar and lunar eclipses occurred in cycles of 6,581 days. If, on the strength of such a purely empirical periodicity, they conjectured when the next solar or lunar eclipse would occur, then they could verify their conjecture or hypothesis directly by waiting till the proper time. This kind of conjecture required comparatively little previous knowledge, and its verification added very little to the existing stock of knowledge, beyond confirming slightly the probable correctness of the assumed periodicity. On the other hand, the modern astronomer dealing, say, with the lunar theory, has a much more complicated task before him. He puts to a severe test all the great astronomical ideas already accepted, and their systematic coherence, which he strengthens or improves by the very use to which he puts them in the long chain of deductions by which he arrives at conclusions that can be tested by actual observation. He must know the approximate masses and the relative positions of the planets of the Solar System; and has to rely on the accuracy of the Law of Universal Gravitation. He must calculate the constant changes in the positions of the earth while the moon is moving round it. And so on. This involves the most elaborate deductive calculations by the aid of differential equations. But when the anticipated positions of the moon, inferred by

the aid of these calculations, are approximately verified, then the whole group of ideas involved is thereby cemented into a coherent system or a "theory," as such an inter-connected system is usually called. Similarly with other theories.

CHAPTER VIII

ORDER IN NATURE AND LAWS OF NATURE

§ 1. *Order in Nature.*

In the preceding chapters reference has been made repeatedly to the order, laws or regularities which science seeks to discover among the phenomena of nature. A few explanations are now called for. When we speak of order or orderliness in nature, or in the world, what we mean, in the first instance, is the opposite of chaos or mere chance. In daily life we have frequent occasion to describe certain occurrences or concurrences as merely chance incidents or coincidences, while other occurrences are not treated as mere matters of chance. When dealing with human actions, the opposite of chance or accident consists of what is usually called design or purpose. Sometimes, for example, we meet friends by chance, at other times designedly, or of set purpose. But, when investigating the vast majority of natural phenomena, we are not concerned with problems of design or purpose. Here the opposite of chance is usually referred to as necessity, which must not, however, be taken to mean the same as compulsion, but simply conformity to a natural law or regularity of some kind. There are two questions then that have to be considered: (*a*)

What is the attitude of science towards order in nature? and (*b*) what is meant by natural laws and regularities!

Science, in seeking to discover order in nature, would appear to assume that there is such a thing. To some extent this is true. If there were no order in nature there would be nothing for science to do. Certainly science does not propose to invent order in nature, or to introduce order into nature, but only to trace it, to discover it, if possible. At the same time, the search for anything does not really or necessarily presuppose the definite assumption or conviction that what is sought is actually there. One may look for what one merely hopes to find, or for what one considers to be there more or less probably. Again, to consider it more or less probable that there is order in nature, is not the same thing as to assume that nature is orderly through and through. After all, the world is vast, and the field of scientific investigation is, in comparison, very limited. The man of science can always select for his investigation a class of facts in which the discovery of order seems promising.

On the whole, experience has shown that there is some order in nature, even if nature be not orderly through and through. If there were no order at all in the world, if the actual distribution of things and attributes and the occurrence of events were entirely a matter of chance, then there would be nothing to exclude any conceivable, or even inconceivable, combinations of attributes or sequences of events. For order expresses itself through laws or regularities.

The whole order of things in any system, whether in nature as a whole, or in any part of it, really consists of a system of inter-connected laws, which constitute as it were the threads of its orderly pattern. Now a law can be expressed in a universal proposition of the form " If S is M, it is P," or, more briefly, if not quite so accurately, in the form, " S is P." And each of these forms of expressing a law excludes certain conceivable combinations, namely, things which are S and M but non-P, in the former case, or things which are S and non-P, in the latter case ($SM\bar{P} = O$, or $S\bar{P} = O$, where $\bar{P}$ stands for non-P).

The complete absence of natural laws, or what comes to the same thing, the complete absence of order in nature would, therefore, show itself in an absence of exclusions of any conceivable combination of attributes or events. What, however, experience plainly shows is that many such conceivable combinations and sequences are not met with. On the other hand, many laws have been discovered which, if true, would definitely exclude various conceivable combinations, the absence of which they may be said to account for. And a considerable amount of experience has so far confirmed these laws. Moreover, the larger the number of laws which science succeeds in discovering, the greater does our confidence become in the extensiveness of the domain of the reign of law, or rather of the pervasiveness of law. True, even the aggregate of human experience is comparatively limited, and our discoveries are always subject to subsequent correction in the light

of further experience. There is no finality in human knowledge, not even in scientific knowledge. But sufficient for the day is the evil thereof. So far the greatest discoveries of the past have not had to be entirely repudiated, only corrected or reformulated.

At the same time, there is *primâ facie* evidence enough of disorder and chance in the universe. Not only are there vast regions of fact in which, as yet, no order has been discovered, but even among the orderly facts or events there are apparently elements of disorder. The individual members of any class of phenomena, for example, usually show considerable deviation from the type, as has already been pointed out in Chapter III, and the most careful measurements of changes subject to natural laws almost always show deviations from one another and from the adopted, or so-called true, value. These deviations (or "errors," as they are often called) cannot be entirely explained away on the ground of human incompetence. They seem to point rather to an element of lawlessness (originality or spontaneity, if you like) in the facts or events themselves.

We may conclude, then, that there are laws in nature, although these laws are not always rigid, but somewhat elastic. There are, however, also parts of nature in which, as yet, no laws have been discovered, and where, so far as one can tell, laws may possibly never be discovered. If we are to use the familiar expression "Uniformity of Nature" to describe the general character of nature, as just discussed, it must not be taken to mean more than

that there are laws or uniform connections in nature, or in many phenomena of nature; and it must not be taken to mean that such laws are absolutely rigid, or that all the phenomena of nature are subject to laws. Least of all may it be interpreted in the sense that the *course* of nature is uniform, or that natural events proceed like recurring decimals. Even if nature were orderly through and through, there would be no need for a perpetual cycle in all natural events. Uniformity of nature is something quite different from the uniform course of nature.

It is possible that order in nature is itself the result of evolution, that in the remote past there may have been even less order than there is now, perhaps no order at all, and that, at some remote future time, nature will exhibit more order than it does now. If so, it would be all the more intelligible why man always looks for order in nature; why he is for ever seeking to discover laws and regularities among natural phenomena, and more so now than he did in the past. His whole attitude, namely, may be the outcome of a growing adaptation to his environment. But this is only a highly speculative suggestion.

§ 2. *Natural Law.*

What is a natural law? Most people are far more familiar with the legal or moral use of the word "law" than with its scientific use, hence a certain amount of confusion. In the legal or moral sense, the expression "law" means a command backed by sanctions and penalties. Usually also,

in these cases, the laws are considered to be imposed upon us from outside by some authority, such as the State, Society, etc. It is quite different from what is called a "natural law." In its scientific sense the word "law" means nothing more than a regularity or uniformity in the character or relation of certain classes of facts or events. It denotes just some intrinsic character, or mode of behaviour, in certain classes of phenomena—nothing imposed on them from outside, but just an orderliness of their own nature.

Another confusion to be guarded against is that between what may be called the law itself, that is to say, the actual objective regularity of the phenomena in question, and, on the other hand, the verbal or symbolic formulation of the law, that is to say, the formula. Unfortunately, the term "law" is frequently used ambiguously, sometimes for the uniformity itself, and sometimes for the formula. What is true of the one may not be at all true of the other. The formulation of a law is the work of men of science. It may require correction from time to time in order to make it express the law or uniformity itself more accurately in the light of increased knowledge. The formula may even have to be rejected altogether, if subsequent research gives ground to suppose that there is no objective law or regularity corresponding to it. But the law or uniformity or regularity itself is usually assumed to be independent of its discovery and formulation. The man of science does not endeavour to invent it, but only to discover it. The formula, moreover,

may only be a close or rough approximation to the law, and in any case it is not the law itself. Natural laws, in short, are not man-made, but only man-discovered and man-formulated. Whatever one's view may be of the ultimate constitution of the universe, or of nature, even if one were to conceive it on idealistic lines, it would be a distortion of the ordinary scientific view of natural laws not to regard them as objective, that is to say, as independent of the discoverer or investigator. We may admit that the scientific formulæ of natural laws are often only approximations, sometimes even deliberate simplifications; but this admission, so far from weakening, actually strengthens the case for conceiving natural law as something strictly objective, a part of nature itself, which the formula does not always express completely.

There are various kinds of natural law. Some of them appear to be more rigid, others less so. Some are almost invariable, others are what may be called regularities rather than more or less rigid uniformities. It is always possible, however, that the inexactness or elasticity of certain natural laws, as conceived by us, may be due to human shortcomings, rather than to the laws themselves, which may simply not be adequately expressed as yet. More important is the distinction between laws of co-existence and laws of sequence. There is a law of co-existence, whenever a number of attributes or states are regularly together. In the case of natural classes, for instance, certain characteristics are usually found together. Similarly, with various

physical states, or the properties of various kinds of geometrical figures. A law of sequence consists of the regular successions of certain states or events, as, for example, between changes of temperature and changes of volume, between thunder and lightning, between the seasons of the year, and so on. Again, some laws are said not to be causal, or are not known to be causal, but are either rational regularities, or merely regularities which may, nevertheless, so far as we can tell, be reducible eventually to as yet unknown causal laws. But the distinction between causal and other regularities, although interesting and really justifiable, or even inevitable, is of no special scientific importance. The really important thing is to discover regularities, of whatever sort. Regularities make the world orderly, and a knowledge of them makes the world, or at least a part of it, intelligible and manageable. For the knowledge of a general law spares us the need of worrying over each particular case. That is an important thing. The question about what may be called the machinery of these regularities is interesting, of course, and we shall, therefore, consider it briefly, but it is only of secondary importance in science.

Another distinction is that between ultimate or primary, and secondary[1] or derivative laws. An ultimate or primary law is one which cannot be deduced from any other law or laws. A derivative or secondary law is one which can be so deduced,

[1] The terms "primary" and "secondary" are here used more or less in the same sense as in the distinction between primary and secondary qualities.

or which is not sufficiently comprehensive to be regarded as primary. For example, Kepler's three laws, already referred to above, are derivative, they can be deduced from the law of gravitation; but the law of gravitation itself is, so far as we can say at present, an ultimate law. The term "ultimate" in this connection must be understood in a relative sense, just like the term "element" in Chemistry. In both cases alike the meaning of the term must be qualified by the reservation "in the light of present knowledge." The expression "law of nature" is sometimes confined to the so-called ultimate laws, but in a wider sense, of course, all natural laws are laws of nature.

§ 3. *Condition and Cause.*

The facts of observation are extremely complex, and in our attempt to trace order in them we have, as it were, to unravel the tangle and follow up the separate threads. That is to say, in every object or event that we observe scientifically, we regard certain of the constituent factors as connected with one another; while others are not considered to be connected with these same factors, though they may be connected with different ones. For example, in the case of falling bodies, we regard the time of the fall as connected with its velocity, but we do not regard the velocity as connected with the chemical constitution of the body, though its chemical constitution will be conceived to be connected with other things, such as its reaction to various agents, and so on. Similarly, in the case of triangles, we

regard the fact that the sum of the three angles is equal to two right angles as connected with the bare trilateralness of the figure, but not connected with the relative lengths of the three sides, though the relative lengths of the three sides will be considered to be connected with something else, namely, the relative sizes of their opposite angles. Now the facts or factors may be said to be connected with one another if one of them is impossible without the other; or, what amounts to the same thing, if one of them is a condition of the other. By a condition of anything, then, is meant whatever is indispensable to that thing or event, etc., or that in the absence of which the thing will not be, or the event will not happen, or at least they will be different. Thus the rotation of the earth is a condition of the sequence of day and night. A certain kind of structure of the stomach of an animal is a condition of its ability to chew the cud, or the equilateralness of a triangle is a condition of its equiangularity. The result, or whatever it is that the condition renders possible, is called the consequent, and every connection may accordingly be described as a connection between a condition and a consequent. Sometimes, as the last example may illustrate, condition and consequent are reciprocal, that is, either renders the other possible; but this is not always or frequently so. Most results are complex, and need the fulfilment of a number of conditions, each of which is indispensable, but is not adequate by itself to complete the result. Thus, for example, the form of a certain curve, the trajectory of a bullet, or the orbit

of a planet is the result of a number of different co-operating conditions.

It should be noted that sometimes the condition and the consequent are simultaneous, and their connection constitutes what has already been described as a law or uniformity of co-existence. In such cases, of course, the term " consequent " must not be interpreted in any temporal sense, but only in a logical or quasi-logical sense, inasmuch as the knowledge of the uniform co-existence of certain attributes enables us to infer from the presence of one of them also the presence of the other. With uniformities of sequence it is different. In their case the consequent really does follow the antecedent, though the connection between them is less frequently reciprocal than in the case of uniformities of co-existence.

Moreover, in the case of the consequents which follow their conditions, we have to take into account negative conditions as well as positive conditions. By a negative condition of any result is meant the absence of whatever may thwart the appearance of that result; while other conditions, the so-called positive conditions, may be said to contribute positively to the result, or to constitute it more or less. Thus, for example, the velocity of falling bodies is conditioned positively by the time of their fall, and not by their mass. Two bodies of different mass or density, say snowflakes and hailstones, should, therefore, fall through the same distance in equal times; but that will not be the case if they fall through a resisting medium, such as air, etc.

Now, the absence of a resisting medium is, therefore, the negative condition of the result contemplated, namely, the fall of different bodies, having different masses or different densities, through equal distances in equal times. Similarly, in the economic sphere, supply and demand and the price of commodities are inter-connected in certain ways, but the actual results may be thwarted or masked by Government control, etc. The absence of such control, etc., is, therefore, a negative condition of the results considered.

Certain sequences of events are commonly considered to be causally connected either directly or indirectly. For instance, an increase of pressure and a fall in temperature would be said to be a direct cause of diminution in the volume, or an increase in the density, of a gas. On the other hand, the seasons, though they follow each other with a certain uniformity, would not be said to cause one another. Rather their sequence would be described as the indirect result of the movement of the earth in relation to the sun, as determined by gravitation and the relative masses and distances of the planets. In this case, a certain uniform sequence, and in other cases, such as those of various kinds of plants, animals and chemical elements, certain uniform co-existences, would be regarded as indirect results of certain causes. By the cause of an event or result is meant the minimum totality of conditions each of which is indispensable and all of which are together just sufficient to bring about that result. It should be pointed out, however, that commonly the

expression "cause" is used in the sense of "condition," or part-cause.

What distinguishes causal connections from other uniformities and regularities is this. The causal conditions are believed to supply the matter and the energy of their consequent or effect, in other words, the event which is the effect of a given cause consists of the matter and the energy of the several conditions composing the cause. The cause has simply been transformed into its effect, or each condition in the cause has been transformed into some part of the effect. The difference between the character of the effect and that of the several conditions, which together cause it, is the result of the action of the several conditions on one another. Now the case is different here from other kinds of uniformities, whether of co-existence or of sequence. In the case of co-existing attributes, the attributes all remain together, there is no question of the conversion of one of them into another. Similarly, with non-causal uniformities of sequence. The seasons, for example, are not conceived to produce one another, or to be transformed one into another. On the other hand, each season considered in relation to its cause is just the result of the light and warmth, etc., which stream upon the earth, when the earth and sun are in certain positions relatively to one another. Thus regarded, it seems legitimate enough to distinguish causal uniformities or laws from others. In the case of physical phenomena they constitute a relation of greater unity and continuity than do those other uniformities which are not

causal, either directly or indirectly. Our knowledge of causal laws in any sphere of inquiry, therefore, facilitates a higher degree of coherence and system in our knowledge of natural phenomena, so as to approximate to some extent to the logical coherence of the mathematical uniformities.

There are some thinkers, however, who object to the use of the term "cause" in science. The idea of causation is derived from the experience of human exertion or effort, and the term "cause" may suggest to some people that the so-called causes, or causal conditions, exert a similar effort in producing their effects. That would be anthropomorphism pure and simple, if not fetishism, or what not. Certainly, such confusions should be avoided as far as possible, but the fear need not be carried to excess. Language is the creation of man, and the tendency to anthropomorphism is its original sin. If science is to avoid every suggestion of anthropomorphism, it must be dumb. Does not the word "law" sin in the same way as cause? And energy? Even the word "routine," a favourite substitute for causation, may be just as misleading. The important thing to guard against is excessive anthropomorphism, whatever terms we employ. After all, it may be possible to carry this fear of anthropomorphism to excess, for man is also a part of nature. Another reason for the objection to the recognition of the causal relation in science is to be found in our inability to see exactly how the so-called causes produce their effects. But even if the accuracy of this reason is admitted, it does not seem to

justify the rejection of the recognition of causal relationship. Does any sensible person deny that he sees the heavenly bodies merely because he does not understand how he does it? However, as already remarked, the important thing is to discover uniformities and regularities in nature, the finer distinctions between them are of secondary importance. In some ways, there is a greater simplicity and continuity in the conception of all regularities and uniformities as constituting a continuous series of correlations from the most imperfect to the most complete. The reduction of the conception of causal connection to that of mere law, or routine, or uniformity may itself be regarded as an expression of the general tendency of modern science towards greater and greater simplifications. On the other hand, the recognition of the causal relationship, as formulated above, seems to be a necessary aid to the conception of the continuity of natural events, and a valuable adjunct to the Principle of the Conservation of Matter and (or) Energy. In the absence of some such conception of causal continuity, each succeeding state of nature would appear to follow the preceding state by a kind of miracle—as, indeed, some of the Schoolmen and others thought it did.

§ 4. *The Principle of Fair Samples.*

All scientific methods start from observed facts, and usually end in generalizations of some kind concerning whole classes of facts, or events, or, at the very least, concerning large groups of them.

The number of phenomena actually observed is usually a small one, in comparison with the whole class or group, of which the results of the application of the methods are held to be true more or less. The question, accordingly, arises, what right have we to apply the results of our observation of a very limited number of facts to others, which have not been observed at all? The answer is that, strictly speaking, we probably have no *a priori* right to do so. Science in this, as in other respects, follows the lead of the practical man. In business it is often impossible or impracticable to examine each item in a large cargo of goods, such as grain, fruit, etc. Buyers are consequently content to estimate the character of the whole cargo by the aid of a sample. They are sometimes taken in; but, on the whole, experience shows that, provided a sample is selected with some care, it fairly represents the whole, although it is but a small fraction of the whole. The precautions to be taken in selecting a sample by which the whole will be judged, is mainly this: to avoid the likelihood of one-sidedness or bias. For example, a sample selected from one or two bags of grain, or cases of fruit, is not so likely to give an approximately correct idea of a large cargo consisting of many thousands of such bags or cases as when the, sample is selected from a considerable number of bags or cases from many different parts of the cargo. Such a selection is commonly described as a *random* selection; but in many cases, perhaps in most cases, the so-called "random" selection calls for a good deal of fore-

thought and insight as to the best way of avoiding bias of any kind. However, samples can be so selected; and, whenever that happens, experience has shown that the character of an adequate or "fair" sample represents the character of the much larger whole with approximate accuracy. Of course, the result of sampling is not absolutely reliable, it can only be regarded as more or less probable, and the degree of its probability will vary with the number and the range of objects constituting the sample. The actual *number* is perhaps less important than the range of selection, that is, the *variety* of objects, wherever there is a *primâ facie* reason to suspect considerable variety in the whole which is to be judged by the sample. Much will depend on the extent of our previous knowledge of the kind of phenomena concerned. In some cases a single instance observed under suitable conditions (such as those characterizing the Method of Difference, for instance) may constitute a fair sample, in other cases a very large sample may not inspire much confidence in its fairness.

As remarked previously, the very attempt to discover a regularity of some kind presumes at least the hope or probable belief that such a regularity is there in the phenomena in which we seek it. To that extent, as already explained, we rely more or less on the principle of the uniformity of nature, in the sense previously indicated. But that principle affords no guidance whatever as to what attributes or variables might or might not be regularly correlated. In actual practice, in science

and in practical life, we rely on what may be called the Principle of Fair Samples, that is to say, the belief that, with reasonable care, it is possible to judge the character of a large group, or of a whole class of phenomena, by the aid of a sample, or a selection, from it. This principle is sometimes called the Law of Statistical Regularity.

CHAPTER IX

SCIENTIFIC EXPLANATION

§ 1. *Explanation and Description.*

Modern men of science appear to be fairly unanimous in maintaining that the object of science is *not* to explain the phenomena of nature, but to describe them. This view is often felt to be rather disappointing, and many thinkers have disputed its accuracy. Sometimes, indeed, men of science not only admit that science does explain, but even maintain that the scientific explanation is the only true explanation of facts. They are usually careful, however, to add at once that in science the word "explanation" means something different from what it means elsewhere. The important thing, obviously, is to get at the actual facts of the case.

The difficulty arises from the fact that things are explained differently on different occasions. The only way to indicate the general function of explanation in such a way as to include all, or nearly all, methods of explanation is to say that anything is explained when it is shown in its relation to some other thing or things, so that it does not appear, so to say, to hang in the air, detached and isolated. (The word "thing," like the word "phenomenon," is here used in the very widest sense so as to include

also attributes, events, etc.) The kind of explanations with which man is most familiar are explanations of the conduct of his fellows, for they are the most important for his well-being. Now, the usual way of accounting for human actions is by relating them with, or referring them to, some motive or purpose. When certain human actions are seen to be the means, or the steps to the realization of some purpose, then we understand them. Our guide to such interpretation is, of course, our own felt experiences on analogous occasions. Such explanations are the explanations with which we are most familiar, and which, perhaps for that very reason, we find most satisfactory. No wonder that man always sought, and sometimes still seeks, such explanations, even when the things to be explained are not human actions. Hence the animism, fetishism, and anthropomorphism in the early history of human thought, and the cheap finalism of even some eighteenth-century thinkers, who seemed so familiar with the intentions of the Almighty! Now, modern science, except in the study of specifically human, and certain other biological problems, does not attempt explanations of this kind at all; that is to say, explanations referring to purposes, or, as they are still sometimes called, *final* causes. In the light of this, it should be clear what is meant when it is maintained that modern science is not concerned with the question *Why?* but only with the question *How?* Science, it is maintained, only seeks to discover what attributes things have, and *how* things happen, not *why*, that is, for what purpose,

things are as they are, or events happen as they happen. And if the term "description" be used for any account of what things are like, and how events happen, then science may be said to be concerned with description. That it is concerned *with description is beyond dispute.* The only question is, whether it is not also concerned with explanation. If the term "explanation" were to be confined to the type of explanation by reference to purpose, or final causes, then science (allowing for the exceptions just mentioned) might be said not to be explanatory, only descriptive. But then explanation by reference to final causes is not the only type of explanation. There are other types of explanation; and it is, indeed, the chief business of science to discover them. Things, attributes and processes, or events, may be explained by reference to their classes, or their conditions, or laws and regularities; and laws and regularities may be similarly explained by reference to other more comprehensive laws, from which they can be derived. Now, we have already seen that the methods of science are directed to the discovery of classes, regularities and laws. To sum up the whole work of science as description is a very inadequate way of indicating its aims and its achievements. At the very least, it is misleading, for the term "description" must, in that case, include all that is usually called "explanation," and the two terms are no longer antithetic.

The idea that science should confine itself to description can be accounted for to some extent in yet

another way. This idea, namely, may be said to mark the climax of a certain revolt, clearly voiced by Francis Bacon and many others in the sixteenth and seventeenth centuries, and to some extent already anticipated by Roger Bacon in the thirteenth century, against the extensive preponderance of speculative theory over actual observation. In the struggle of those early days it was urged, and rightly urged, that science must rest on the observation of facts, and not on the theories of authorities. The only authority for science must be the observed facts, and the rational interpretation of the observed facts. In the positivism of the eighteenth century this tendency reached its extreme form in the demand that science should shun philosophical as well as theological authorities, should shun, in fact, theoretical speculation altogether, and confine itself to the description of actual observations. The watchword of positivism has met with almost universal favour among men of science. The spirit which prompted this whole tendency was healthy. It is right that science should keep as close as possible to observed facts, and not indulge in unnecessary speculation. But this tendency may also be carried too far. What is commonly called observation includes, over and above actual sense-elements, not only such supplements of memory and imagery as make wool look soft, or ice look hard and cold, but also distinct elements of interpretation. This is not usually noticed, because the interpretation is so rapid and spontaneous that the sense-elements and the interpretations coalesce into one experience, the whole

of which appears to be given immediately. That there is an element of interpretation even in observation becomes clear in the case of conflicting descriptions of the same objects or events, as frequently happens in the Law Courts. For example, in a certain trial, one witness maintained that he had noticed on the seashore in the moonlight a woman with a child, while another witness of the same scene was equally certain that it was a man with a dog. For the most part the rapid interpretations we make in ordinary everyday life are correct. We do, indeed, make mistakes sometimes, and grow more cautious in time; but we could not check the process entirely without paralysing our intellectual and practical life. Now, what is true of ordinary experience is true more or less also of science. Description, if it is to be sharply distinguished from explanation, should be confined to what is actually observed. In reality, however, it is as difficult to separate entirely description from explanation as it is to separate entirely observation from interpretation. For scientific explanation is a form of interpretation, or rather consists of several forms of interpretation. The contention that science is not concerned with explanation at all can only be misleading, and may be partly responsible for the occasional tendency to smuggle explanations into descriptions, or to pass off theories for facts. The distinction between fact and theory, as commonly made, is, indeed, only another form of the distinction between observation and interpretation, or between description and explanation. Strictly speaking, if

the distinction is to be carried through consistently, the term "fact" should mean what can actually be observed, as distinguished from the theory which links up or explains the observed facts. But only too frequently the word "fact" is applied to anything, even a highly abstract theory, if it is believed with as much conviction as anything that is perceived ("fact" in the stricter sense). The only consistent course is to recognize that science is concerned with interpretation as well as with observation, with theories as well as with facts, with explanations as well as with descriptions; and to admit, besides, that it is not always easy or necessary to distinguish sharply between the terms in each antithetic pair.

§ 2. *Types of Explanation.*

The aim of science is to discover order in the world, and all the scientific methods are methods of tracing order among various natural phenomena. In so far as science succeeds in its enterprise, the world, or at least some part or aspect of it, appears to us more orderly or intelligible, or more explained. To explain anything is to see, or to indicate, its place in some order of things or events. Hence various methods of scientific explanation, corresponding more or less to the principal scientific methods of discovering order in nature. The chief types of explanation may be enumerated and illustrated as follows:—

(*a*) *Reference to Class.* An object is sometimes explained when it is recognized, or shown, to be a

member of a known class. Thus, for instance, to one who is in doubt about the character of a certain plant, it will be explained if (whether by the aid of an analytical key, or through the help of an expert) he finds out that it belongs to such or such a species or variety. Similarly, a class of objects may be explained when it is recognized as a sub-class of a wider class. Events also are sometimes explained in this way, as, for example, when lightning is classed with electrical phenomena. Of course, to anyone who knows nothing about the kind of objects, or class of events, to which reference is made, this is no real explanation.

(*b*) *Reference to Evolutionary Series.* A type of object is sometimes explained by reference to its place as a link in an evolutionary series. (It makes no difference whether a type is represented by the fossil remains of a single member, say, the South African skull of an ape-man, discovered by Prof. Dart, or by a multitude of surviving members.) In such a case something which is at once similar and yet dissimilar, when compared with other types, and therefore puzzling, is assigned its place in a certain order of continuous development, and is thus explained. Similarly, a whole evolutionary series may be explained by ascertaining or indicating its place in a more comprehensive evolutionary series.

(*c*) *Reference to Mediating Factors.* When the problems concern apparently remote or diverse facts, or events, which, nevertheless, appear to be connected, then an explanation may take the form of discovering, or indicating, intermediate factors or

events, which bring the correlated, but remote, facts or events into closer connection. Thus, for example, the perception of sound is explained by the mediation of air-waves between the source of sound and the hearer. Similarly, the perception of luminous objects is explained by the mediation of ether-waves between the luminous object and the seer. And the correlation between the presence of cats and the abundance of clover is explained by reference to such intermediate events as the cats' destruction of the mice that would destroy the bees, which fertilize the clover.

(*d*) *Reference to Laws.* The commonest type of explanation consists in referring what needs explanation to some relevant law or laws. The laws may be partial or complete correlations, and, if complete correlations, they may be merely empirical uniformities or causal or logical connections. Thus the frequent occurrence of suicides in a certain city, or country, may be explained, after a fashion, by reference to the approximately constant rate of suicides there. The bent appearance of a stick partly immersed in water may be explained by reference to Snell's Law of Refraction. The successive positions of a planet may be explained by reference to Kepler's 1st or 2nd Law, or both. The movements of a planet, or Kepler's Three Laws, might be explained by reference to the Law of Gravitation. Lastly, the equiangularity of a triangle may be explained by reference to its equilateralness, or *vice versa*. The most satisfactory scientific explanations are those based on causal or logical

uniformities. Other laws, indeed, are felt to need explanation themselves, and the attempt is usually made to explain them by reference to causal laws, or theories.

(*e*) *Reference to Purpose.* In the study of certain biological phenomena, and, above all, in the study of human experiences and activities, individual and social, it is scarcely possible to dispense with the conception of purpose, if we are to have really adequate explanations. Even the most violent opponent of teleological explanation, even the most thoroughgoing determinist, would hardly be flattered if his writing and other activities were described as guided by no aim, and devoid of all purpose! Still, even these special problems must be explained in other ways, as well as teleologically; and in the case of the purely physical sciences teleological explanation has no place.

§ 3. *Theory and Law.*

Laws, if they can be explained at all, are explained by reference to wider laws, as has already been pointed out. But the converse is not always true. The reference of a law to a wider law does not always yield an explanation. In order to make this point clear it is necessary to distinguish first between real inductions and mere summaries. If, after examining a sufficient number of various animals having cloven hoofs, and noticing that they are ruminants, we conclude that all animals with cloven hoofs are ruminants, then we have an induction or generalization. It may, or may not, be justified, still it is a

real inference from observation to a law or uniformity. But if we confine ourselves to the statement that "all the animals with cloven hoofs which we have examined were ruminants," then we have merely a summary account of the actual observations, which involves no such inference as the previous statement. It formulates no law. Similarly with statements made about a limited group or class after examining each member separately. I mean such statements as "No month has 32 days," or "All the Apostles were Jews." These are merely summary statements. Such summaries are very useful as aids to memory, and for purposes of easy reference; but they are not inductions, although they have been described as *perfect* inductions, that is to say, inductions based on perfect (= complete) enumeration. Now, laws, too, may be summarized in such a manner, and when they are so summarized, the result may be called a summary law, and will, of course, be more comprehensive than any one of the laws which it summarizes. But it is not a new induction, for it includes nothing that is not already included in the laws which it summarizes. For example, if, after experiment with some samples of a particular gas, it is inferred that in all cases of that kind of gas the pressure and the volume vary inversely, if the temperature remains constant, then there is an induction. But if, after making similar inductions about each known kind of gas in turn, it is asserted that "the pressure and the volume of any gas vary inversely" (Boyle's law), then the statement is a summary of the

separate inductions; it is not an additional induction. Similarly, if the Law of Refraction of Light (or the Index of Refraction) for each kind of transparent medium were ascertained first, and then all the separate inductions were summed up in Snell's Law of Refraction. It might be urged that such summary laws (like Boyle's Law or Snell's Law) are really genuine inductions, because they also apply to certain kinds of substances which may be discovered in future (say, new gases, or new refractive media). But it is not very likely that such laws would really be assumed to hold good of newly discovered substances without experimental verification. This means that the relevant summary law would be treated as an hypothesis (suggested by analogy), and tested like any other scientific hypothesis. If verified, it is a new induction, and can then be included in the relevant summary induction.

Now, a summary law does not explain any of the included laws. It is only when the more comprehensive law is something more than a summary that it can be said to account for other laws which can be derived from it. Thus, for example, Newton's theory of gravitation (especially in its original causal sense) is an explanation of Kepler's three laws and of Galilei's Law of Falling Bodies; the Kinetic Theory of Gases is an explanation of Boyle's, Avogadro's and Gay-Lusac's Laws (and of the separate laws which they summarize); and the Undulatory Theory of Light explains Snell's Law of Refraction (and the laws of which this is a summary) by reducing the bending of a ray of light, as it

passes from one medium to another of different density, to differences in the velocities of light in the two media.

There is a tendency to distinguish such a more comprehensive law from the less comprehensive laws, which it explains, by calling it a *Theory*. In the preceding paragraph this distinction in nomenclature has been observed. It is a convenient distinction. The reason why these more comprehensive laws are called theories, rather than laws, may be justified as follows: In the first place, they are usually of a more speculative character than the subsidiary laws which they include, and in that sense they are more theoretical. The subsidiary or secondary laws keep more closely to the facts, and are more nearly descriptive than are the so-called theories. The theories are, in a sense, inductions from the laws, in the same way as the laws are inductions from the facts of observation. The theories are, consequently, at a further remove from the facts than are the laws; the laws may be true even if the theories are false, but the theories cannot be true if their derivative laws are false. The theories are, therefore, less probable than the laws, according to the principles of probability explained in Chapter X, § 2. Another reason may be this: Looking at the laws objectively, that is to say, as natural laws, and not merely as verbal formulas for them, it is fairly obvious that to call the theories also laws amounts to counting the same laws twice over. The actual uniformities in the phenomena concerned may be accurately described either by

reference to the secondary laws or by reference to the systematizing theory. But the two do not represent different uniformities; and, since the theories are the more speculative, and also come after the discovery of the laws, the term "law" is naturally retained for the less comprehensive but earlier discoveries. Incidentally, the foregoing explanation may also account for the fact that we usually speak of *discovering* a law and *inventing* a theory. The word "theory" suggests at once a formula, or a formulated explanation or hypothesis, rather than an objective uniformity. But the reverse is the case with the word "law."

The invention of theories, in the sense just explained, marks an important step forward in the history of a science. For theories colligate secondary laws, just as laws colligate or order facts. Theories, therefore, mark further progress in the discovery of systematic order in the phenomena of nature. Laws of a less comprehensive character, which cannot be deduced from theories, are felt to be unexplained, to hang in the air, so to say, and are often referred to as merely *empirical* laws, in the sense, namely, that they only sum up, or just describe, the experienced or observed facts, without explaining them adequately. It should be remarked, however, that, at other times, the expression "empirical law" is confined to inductions which are based on simple enumeration, and which are, therefore, not so reliable as inductions based on the stricter inductive methods. (See Chapter VI, § 1, above.)

§ 4. *The Validity of Science.*

Science, like all knowledge, is based partly on observation and partly on inference, and both these processes are exposed to error. Hallucinations, and illusions, and fallacies are common enough to warn any sensible person against excessive confidence in his views. Even science, in spite of all the caution taken in its construction, is not infallible, for, in addition to the possible sources of error just indicated, there is an element of uncertainty inherent in the very character of inductive procedure, which plays an all-important rôle in science. Science usually proceeds, and rightly proceeds, on the assumption that the phenomena which we observe are probably in some sense the products of conditions operating according to laws and regularities of some kind; and the business of science is to discover these conditions and laws. The procedure is of the nature of what is known as an *inverse* or reverse process, and may be compared, to some extent, with that of ascertaining the factors which may have produced a certain numerical or algebraic product. To such a problem there is usually more than one answer possible, and all that can be done is to enumerate all the possible answers. The case of natural phenomena is much more difficult. It is not possible, in this case, to enumerate all possible answers. The purely quantitative part of a suggested natural law may be determined with comparative certainty, but the rest of the solution is always liable to challenge by a rival solution, if not now, then later on.

Still, this is no reason for scepticism. One can only test hypotheses that have actually been suggested, and embrace the one that best survives the ordeal of verification. How can one examine hypotheses not yet suggested? One can only cultivate a sufficiently open mind to pay due consideration to a better hypothesis, if and when it is put forward. Generally speaking, an hypothesis that has stood a long and severe test only calls for modification, rather than for rejection, even when it does eventually break down. So the scientific results gradually accumulated by generations of workers may well be accepted with the confidence that is placed in what is highly probable, if not with that absolute certainty which is reserved for what is beyond all doubt. The uncertainty which attaches even to scientific knowledge has prompted some people to search for a starting-point or principle that is established beyond all doubt, and which may serve as the sure foundation of further knowledge based upon it. But this kind of Archimedean fulcrum, for the raising of science, has not yet been discovered. For the most part, observation is regarded as the safest basis of knowledge. In a wide sense the popular proverb "seeing is believing" is commonly endorsed in science as well as in everyday life. If we cannot believe what we perceive, what *shall* we believe? Scientific knowledge consists mainly of what is known from observation and what is believed to be inferable from what has been observed. Even observation, however, is not infallible. It has already been indicated that, even apart from hallucination,

observation or perception usually includes elements of interpretation or explanation, which may be wrong. The line between what is called observation, on the one hand, and avowed explanation, on the other, or between so-called facts, on the one hand, and avowed theories, on the other, is not always easy to draw. The difference between them is mostly a difference of degree, rather than a difference of kind. Even the reliability of observation depends in a measure on our calling up the correct interpretative elements to blend with what is given in sense-impression; and we do not always succeed in doing so. There would thus appear to be no indubitable starting-point for knowledge. But in practical life and in science we do not worry about the apparent absence of such an immovable foundation. What actually happens may be suggested by a parable. The ancient Indians sought for a sure foundation for the earth, and so they suggested that the earth rested on an elephant. But the elephant likewise, it was felt, needed a sure resting-place, and so it was suggested that the elephant stood on a tortoise. Now we have long since abandoned this kind of search for an elephant or a tortoise on which the earth might rest securely. We are quite content with the idea that the earth and the other planets, or indeed all the stars, sustain one another gravitationally in such a manner that they can all move freely and safely in their courses, without any risk of tumbling down. So it is with our beliefs: we put our faith in the co-operation or the convergence of our observations and other judgments or beliefs.

We do not suspect all observations simply because some of them have proved to be unreliable. For the most part, we believe what we observe, and we only doubt an observation when it conflicts with other experiences. If this is true already of observation, it applies with even greater force to inference, and especially to the inductive inferences by which science is more especially built up. None of the inductive methods can be applied so rigorously as to escape all cavil, and some of them are not very satisfactory at the best. The generalizations which rest on the Method of Simple Enumeration, for instance, always have a low degree of probability. The greater the number of observations on which they are based, the greater is their probability; but the probability is never very high. The other inductive methods depend for their reliability on our ability to detect all the relevant circumstances, and to vary these as much as possible. Now, the investigator may fall short in both these respects. In ignoring certain circumstances as irrelevant he relies more or less on his previous knowledge of them, and that knowledge also is not beyond cavil. Yet, it is only by relying on previous knowledge that an investigation can be kept within manageable bounds. Similarly with the range of variability of all the relevant circumstances. The greater the range of variation of one relevant circumstance at a time, the more probable is the conclusion concerning the connection between a certain condition and a certain consequent that is based upon it, in accordance with one or other of the simpler inductive

methods. But it is rarely possible to vary just one relevant circumstance at a time. How then, it may be asked, does science ever get a start? The answer is similar to that already given above with reference to observation. We rely on the harmony or mutual support of the whole of our knowledge. If the new conclusion harmonizes with the rest of our knowledge or our beliefs we accept it; if not, we sometimes reject it, and sometimes we readjust our previous beliefs in such a way that the new belief and the refashioned old beliefs should be consistent. In this way, old knowledge promotes the acquisition of new knowledge, while the new knowledge helps either to confirm or to correct old beliefs. And, as human experience grows more and more extensive, and human knowledge becomes more and more comprehensive, and embraces vast ranges of experience colligated into self-consistent systems, which also harmonize with one another, so science gains in probability, and approximates nearer and nearer to certainty, even if it should never quite reach it. The nature of probability will be considered in the next chapter.

CHAPTER X

PROBABILITY

§ 1. *The General Nature of Probability.*

Probability is usually contrasted with certainty, and both terms apply only either to judgments or to propositions (which are the verbal expressions of judgments). There are cases in which we do not feel competent to judge at all, when we simply suspend judgment, and then the question of probability or certainty does not arise. But when we do judge, then the judgment is entertained either with relatively complete confidence, which is called certainty, or with some lesser degree of confidence. It is this lesser degree of confidence, the confidence that falls short of certainty, that is usually designated as probability. It is more convenient, however, to regard complete confidence or certainty as the limiting case of probability. We then have a continuous series or scale of probability varying from the lowest to the highest degree of confidence.

To avoid possible misunderstandings several things must be borne in mind. Certainty or uncertainty may originate in different ways. It may be the result of the moods and dispositions, hopes and fears, habits and prejudices of the individual who is judging. These are subjective, personal factors,

which vary from individual to individual. Some people confidently expect a certain event merely because they wish it to happen, and they are sanguine by temperament. Others may be extremely uncertain about some event, either because they are not keen about it, or because they have a morbid habit of expecting the fates always to thwart their wishes. In contrast with such merely subjective causes of certainty or uncertainty, there are objective or logical grounds on which they may be based. They are the kind of grounds to which we usually appeal when we try to convert others to our views, and do not rely entirely on our powers of bullying or of coaxing. Such rational grounds do not vary from individual to individual, but are valid for all intelligent beings. Now, the kind of uncertainty with which probability is concerned is that based on rational grounds. It is not concerned with mere feelings of conviction arising we know not how, but with those varying degrees of assertiveness which are correlated with corresponding degrees of rational support which our judgments find in the available evidence.

Hence, probability may be said to be concerned with the problem of *rational* belief. Many of our judgments we are quite sure of, without any inspiration from our feelings or prejudices; sometimes indeed, in spite of them. "The square on the hypotenuse of a right-angled triangle, is equal to the sum of the squares on the other sides," however much one may dislike the Pythagorean theorem. On the other hand, there are many things about which we

cannot judge with certainty. We may have some grounds for thinking that "*S* is *P*," but the grounds may not be conclusive. We must then content ourselves with the judgment, "*S* may be *P*," or "*S* is probably *P*." It may be as well to emphasize at once the fact that probability and certainty refer to judgments about things or events, not to things or events themselves. Language, in its commendable aim at brevity, is rather misleading sometimes. Things just are what they are, and events just happen as they do. It is only our judgments about them that can be either probable or certain. It would indicate no additional character in the thing or in the event referred to in the statement, "snow is white," or "the water is freezing," if one were to insert the word "certainly" or "probably" after the word "is." The insertion of either of these words would only indicate a difference in the degree of our confidence in our own judgments, nothing else. As a matter of convenience, however, one may and does speak of "the probability of events," when what one really means is "the probability of the judgment that the events will happen"—much in the same way as one continues to speak of "sunrise" and "sunset," etc., and would consider it tiresome pedantry to have to express himself accurately in accordance with the heliocentric astronomy.

Again, propositions which are the result of direct observation are normally entertained with certainty. It is only when we are observing under difficulties that we become uncertain, and then the uncertainty

attaches not so much to the sense-impressions as to the elements of interpretation which enter into the complex whole of our perception. Generally speaking, therefore, it may be said that the question of probability arises chiefly in connection with inferred judgments, that is, judgments which rest on evidence; and degrees of probability may then be said to be correlated with degrees of evidence, or degrees of cogency in the evidence. Accordingly, there are as many degrees of probability as there are kinds of available evidence. Moreover, the probability of our judgment relating to the same things may vary considerably from time to time, as more and more evidence comes to light. Our increasing knowledge of the evidence may, of course, have no influence on the thing or event in question; but it is all-important in determining the rational justification of our judgments relating to it.

In most of the affairs of life we have to come to a decision on evidence which is not conclusive, so that our judgments are not certain, only more or less probable. "Probability," as Bishop Butler has said, "is the very guide of life." This very fact has imparted a special interest to the study of probability and the problem of its accurate measurement. Indeed, the mathematical treatment of probability is concerned almost exclusively with the measurements of probability. But not all cases of probability are really measurable, not even all those cases in which differences of degree are readily distinguishable. For example, in a law-court one may feel justified in believing it to be more likely that

witness A had told the truth rather than witness B, and yet one may be unable to assign a definite truth-probability to the statement of either. Or a witness may think it more likely that a certain event had happened at one time than at another, and yet he may be unable to estimate the probability of either. No doubt it is very trying to have to depend on such vague estimates as "rather probable," "quite probable," "very probable," etc., especially in the course of trials on which issues of life and death may depend. One can understand, accordingly, the motive behind Bentham's suggestion for the use of a probability-scale on which witnesses and judges might indicate the degrees of certainty (from 0 to 10) of their evidence or conclusions. But it is difficult to see the practicability of the suggestion. With unconscious humour the proposed probability-scale has been called a probability-*thermometer*. If adopted, it would probably serve to indicate more often warmth of feeling than the dry light of reason.

There are cases, however, in which probability may be measured with great precision. It is these cases that have chiefly interested writers on Probability, as well as those ardent wooers of Fortune who seek to reach her by short cuts. These measurable cases are of two principal types, namely, those which can be calculated *a priori*, and those which can only be calculated *a posteriori*. By the *a priori* cases we mean those which can be determined by reasoning from the nature of the case, independently of actual observations of the kind of events contemplated.

By the *a posteriori* cases we mean those which can only be determined by the aid of such observations.

§ 2. *The Deductive Calculation of Probability.*

The *a priori* (or deductive) method of calculating probabilities is possible on the following conditions: (1) We must know the total number of mutually exclusive alternatives, one or other of which must happen. (2) These alternatives must be equally likely. (3) We must know how many of the alternatives are favourable to the event concerned. The probability is then expressed by means of a fraction, the denominator of which gives the total number of equally likely alternatives, while the numerator gives the number of alternatives which are favourable to the event in question. The general formula may be expressed thus: $p = f/t$, where p stands for the degree of probability, t for the total number of equally likely alternatives, and f for the number of favourable alternatives.

This mode of assessing probability at once suggests quantitative expressions for certainty, as limiting cases of probability. There are two cases of certainty, namely, when we know that something is necessary, and when we know that something is impossible. We are just as certain that equilateral triangles cannot be rightangled as that they must be equiangular. But, owing to the habit of applying the terms probability and certainty to the events, instead of to our judgments about them, it is usual to employ the term "certainty" for only one of its two forms, namely, necessity, the other form being

separately designated as "impossibility." One does not want to be pedantic in the use of words, and so long as it is remembered that impossibility is also a case of certainty, in the wider sense and more correct use of the term, there is no harm in conforming to current usage. Impossibility means that none of the possible alternatives is favourable. Its probability is, therefore, o/t, which $= 0$. For example, it is impossible to throw an ace with a coin, for there is no ace on a coin, only head and tail. Similarly, it is impossible to throw head with a die, for a die has no head, only facets marked from 1 to 6. The probability is, therefore, $0/2$ and $0/6$ respectively, that is 0, in either case. Certainty, on the other hand, means that the event contemplated must happen, either because it is the only possibility under the given conditions, or because, although there are a number of different alternatives, yet any one of them will serve the purpose in hand, that is to say, will be favourable to the result in question. In either case all the alternatives are favourable, so that in this limiting case $p = t/t = 1$. In this way, starting from the formula $p = f/t$ we can deduce from it the limiting probability-values of impossibility and certainty as 0 and 1 respectively. So that the values of probabilities proper must be more than 0 and less than 1. In the two limiting cases it is not even necessary to stipulate that the alternatives should be equally likely.

In the case of simple events it is usually quite easy to determine the values of t and f. Thus, for example, the probability of throwing head with

a properly constructed coin is 1/2. That of throwing a 6 with a well-balanced die is 1/6; that of throwing 1 or 6 is 2/6 or 1/3; that of throwing an even number is 3/6 or 1/2; and so on. In the case of complex events, however, the task is somewhat more difficult, and calls for some caution. By a complex or compound event is meant one in which two or more separate events can be distinguished. Now, the total number of possibilities in such cases does not consist of the sum of the possibilities of the separate events but of their product. Moreover, each possibility is not something simple; it is complex, and must be expressed in terms of all the component events. Both these points must be borne in mind if mistakes are to be avoided. For instance, if a die is thrown twice (or two dice are thrown simultaneously) the total number of possibilities is not $6 + 6 = 12$, but $6 \times 6 = 36$, because for each possibility with one die or throw, there are six possibilities with the other; therefore altogether there are 6×6 possibilities. Sometimes, the sum of the separate possibilities is equal to their product. In the case of two coins, for example, the sum of their possibilities is $2 + 2$, and the product is 2×2, and the two are equal. In such a case, it is especially important to remember the second of the above points, namely, the complexity of all the possibilities. Suppose, for example, we want to know the probability of obtaining head in one or other of two throws with one coin, or with two coins thrown together. We might argue plausibly that there are four possibilities, namely, head or tail on the

one coin, and head or tail on the other coin—h_1, t_1, and h_2 or t_2—and that two out of the four possibilities are favourable; therefore, $p = 2/4 = 1/2$. But that would be wrong. There are four possibilities, it is true, but they are all complex, not simple like those just given. The four correct possibilities are (1) head on both coins (or in both throws); (2) head on the first and tail on the second; (3) tail on the first and head on the second; (4) tail on both—$h_1\ h_2$, $h_1\ t_2$, $t_1\ h_2$, $t_1\ t_2$. Of these four possibilities three are favourable (the only unfavourable one being that in which tail is thrown in both), and therefore the true probability is 3/4. In the case of two dice thrown simultaneously (or two throws with the same die) the correct possibilities will be as follows, if we let the first digit in each number stand for the number appearing on the first die (or in the first throw), and the second digit for the number appearing on the second:—

11	12	13	14	15	16
21	22	23	24	25	26
31	32	33	34	35	36
41	42	43	44	45	46
51	52	53	54	55	56
61	62	63	64	65	66

Now the probability of a compound event, as compared with the probability of the separate component events, may be either greater or less, according to circumstances. If, in the case of the complex event, all that is required is any one of the component events, then the probability of the complex event is greater than that of any of its simple components.

For example, we have just seen that the probability of obtaining head in either of two throws of the coin is 3/4, which is greater than that of getting it in one throw only, which is 1/2. Similarly, the probability of getting a 6 in either of two throws of a die is greater than that of getting it in one throw only, namely, 11/36 as against 1/6 (see the foregoing table). The reason for this is fairly clear. When a second throw is permissible, then we may still get what is required, in the second throw, even if we have missed it in the first throw. Therefore the second throw increases the probability. But the permissibility of the second throw does not double the possibility of the one throw only, because the permissibility of a second throw will have no value if we obtain what is required in the first throw. The case of $h_1 h_2$ in the case of the coins, or that of 66 in the case of the dice, must not be counted twice.

On the other hand, the probability of the compound event may be less than that of the simple events which compose it. This happens whenever the compound event contemplated is one in which certain component events must all be there in a certain order. It is obvious, on general grounds, that the probability of both events, A and B, happening is less than that of A alone or B alone happening, because either of them might happen *without* the other, as well as *with* the other. Thus, for instance, it is less probable that head will appear in both of two throws with a coin than in one throw, because we may fail to get head in the second throw even if we get it in the first throw. Now, in such cases, the

probability of the compound event is obtained by multiplying the fractions expressing the separate probabilities of the several component events. Thus, the probability of throwing head in both of two throws of a coin is

$$1/2 \times 1/2 = 1/4\,;$$

that of getting 6 in both of two throws of a die is

$$1/6 \times 1/6 = 1/36\,;$$

that of getting an even number in both throws is

$$3/6 \times 3/6 = 9/36 = 1/4\,;$$

and so on. The reason for the multiplication of the separate possibilities in this type of case may be stated as follows: The event contemplated is one in which several component events, say *A* and *B*, must all occur. Now, unless *A* does occur it makes no difference whether *B* occurs or not; it is of no interest to us then. Therefore, *B* will only be taken into account if *A* does happen. It is consequently not certain, but only probable, that *B* will be taken into account at all. How probable? As probable as that *A* will happen, say p_a. Now, if *A* does happen, then *B* is taken into account; but it is not certain that *B* will happen even then. There is only a probability of its happening even as a separate event, say a probability of p_b. Hence, the total probability of *B* happening precisely as required, namely, when *A* also happens, is $p_a \times p_b$. Similarly with more complicated events.

What has just been said about compound events and their being less or more probable, according to

circumstances, than the separate component events applies also to events or things whose probability cannot be calculated at all. Thus, for example, it is more probable that the weather will be fine on Monday *or* Tuesday than on Monday; but it is less probable that it will be fine on both Monday *and* Tuesday than on Monday. Incidentally, this will explain to some extent why some hypotheses are less probable than others. The wave theory of sound, for instance, is, in the light of the foregoing considerations, more probable than is the wave theory of light, because the latter assumes two things, namely, the existence of ether and wave-transmission, whereas the former theory assumes only the wave transmission, since its medium (air) is known to exist, and is not a matter of assumption.

§ 3. *Equally likely Possibilities.*

To return to the above-mentioned conditions for the *a priori* calculation of probability. The second of those conditions calls for special consideration. First, why must the alternatives be equally likely? A comparison of two simple cases will make the point clear. Suppose we were to argue that a properly constructed coin must either throw head or not; that there are, therefore, two alternatives, of which head is one; so that its probability must be 1/2. The answer would be true, but the reasoning would not be correct. Let us now take a die instead. Suppose we were to argue that the die must either throw ace or not; that there are, therefore, two alternatives, of which ace is one; so that its proba-

bility must be likewise 1/2. Here the answer is palpably wrong. But why? Is it not true that the die must either throw ace or not? Are there not these two alternatives? In a sense there are two alternatives in the case of the die as well as in the case of the coin. But whereas in the case of the coin the alternative "not-head" is equal to the alternative "head," in the case of the die the alternative "not-ace" really stands for five separate alternatives grouped together as one alternative—it is really five times as great as the alternative "ace," and must be weighted accordingly. Its probability is really 5/6, while that of the ace is only 1/6. In the same way, the alternative "bull's-eye" is not comparable with the alternative "not bull's-eye," for the former represents but one comparatively small space on the target, while there are innumerable places on and off the target which are "not bull's-eye." Similarly with the happy mean in right conduct, as Aristotle would say. Incidentally, this will also explain the absurdity of the view that a statement for which there is no evidence *pro* or *con* has a truth-probability of 1/2. There is no reason whatever for supposing that the truth and the falsity of such a statement are equally likely alternatives. There are innumerable ways of missing the truth, just as there are innumerable ways of missing the mark.

Next, the condition appears to be question-begging. Apparently we must know that the alternatives are equally probable before we can determine the probability of any one of them. If

so, how is one ever to get a start? Poincaré, accordingly, had no difficulty in poking innocent fun at the whole calculus of probability, which he considered to be little better than a game conducted in accordance with certain rules and conventions. But it is an exaggeration to regard the equality of the alternatives as a mere matter of convention. The equality of the alternatives may be ascertained without the aid of fallacious question-begging or arbitrary conventions. Let us suppose that we have witnessed the construction of a die made as nearly perfect as possible. What do we know in that case, and what are we justified in anticipating from a throw of the die? We know that the principle of gravitation and the laws of centre of gravity and of specific gravity are operative, and that the six sides of the die are approximately equal in shape and in weight. From all this we deduce with comparative certainty that the die when cast will not remain suspended in mid-air, or stand up on one of its edges, or remain poised on one of its corners, but must fall flat on one of its sides. But which side? That we cannot tell, because there are six equally likely alternative sides. In what sense are the alternatives equally likely? In the sense that the conditions which are known to be operative are known to be equally favourable to any one of the six sides of the die. Other conditions, namely, the precise way in which the die will be thrown, etc., will have to become operative in order to make the die fall on one side rather than on any of the other sides. But, since these conditions are unknown, we can only be guided

by the known conditions, which are equally favourable to each of the six sides. In this sense the six alternatives are equally likely. That is all that is required by the condition under consideration, so that no convention is necessary, nor is question-begging involved. It is not always possible to ascertain the equal likelihood of the alternatives in such a direct way, and then one is tempted to let mere ignorance of inequality do service for a knowledge of equality, a procedure that is not always justifiable. The equality of alternative possibilities can sometimes be determined indirectly. Suppose, for example, we have a die of the proper construction of which we have no evidence, and which we do not want to take to pieces in order to examine its sides, we might still ascertain indirectly whether its six sides are equally likely alternatives. We might, namely, cast the die several thousand times and record the results. If, on an average, each side of the die appeared approximately once in six throws, then we should feel justified in regarding the alternatives as equally likely. Observations of the actual behaviour of the alternatives are thus made a test of the equal likelihood of those alternatives. This brings us to another method of estimating probability—the *a posteriori* (or inductive) method.

§ 4. *The Inductive Calculation of Probability.*

Many, and practically the most important, cases of probability cannot be calculated *a priori* at all; but they can be estimated by the aid of sufficiently

numerous observations of the class of events contemplated. Suppose, for instance, that, in the above case of the die, the six different sides were not thrown an approximately equal number of times—the ace, say, turning up on an average once in about eight throws, instead of once in six. This would be taken to prove that the six alternatives are not equally likely, so that the probability of any particular throw with that die could not be calculated *a priori.* But it could still be calculated by the aid of the actual observations—ace, e.g., would be said to have, a probability of 1/8. The frequency with which an event occurs, in the long run, is treated as the measure of its probability. In this way, provided we have sufficient statistical data, it is possible to estimate exactly the probability of all sorts of events which do not lend themselves at all to *a priori* treatment—births, marriages, deaths, the thousand-and-one ills that flesh is heir to, etc.

Influenced by the empirical tendencies of a scientific age, and contemptuous of the high *a priori* road favoured of theologians and philosophers, writers on Probability have been tempted to base all calculations of probability on frequencies. They would either banish *a priori* calculations altogether, or, at most, treat them as intelligent anticipations of frequencies. It is admitted, indeed, that the frequency-theory of Probability is not free from difficulties. Frequencies are apt to vary considerably with the number of cases observed. In tossing a coin, for example, the proportion of heads and tails varies remarkably according as one stops

at the 50th, the 100th, the 1,000th, or 10,000th throw. One can get almost any proportion by stopping at the right moment. Hence the introduction of the notion of "the long run"—*in the long run* a normal coin will throw head once in two throws. But even so, the element of arbitrariness is not entirely eliminated. As here conceived, the fundamental form of the probability-calculus is the *a priori* form, of which the *a posteriori* type is simply an inverse process. The probability that a die, which is known to be properly constructed, will throw ace is 1/6. Primarily this fraction does not refer to the average frequency with which ace has turned up or will turn up. It means that ace is one of six equally likely alternatives, one or other of which must be realized, should the die be cast. This may be ascertained accurately without casting the die at all. The said frequency may, however, be deduced from the *a priori* probability, of which it can, consequently, be made a test or an index. In the *a posteriori* calculations we simply say that the event in question occurs *as if it were* one of so-and-so many equally likely alternatives—the ace of the above-mentioned bad die, e.g., turns up as if it were one of eight equally likely alternatives. There is nothing unusual about such a use of the inverse process, or the resort to fictitious suppositions.

Our view of the calculable cases of probability (*a priori* and *a posteriori*) makes it possible, without any straining, to keep together all types of probability, quantitative and non-quantitative. In all

cases of probability proper the certainty of anticipation is weakened by our knowledge of alternative possibilities. In some cases we know directly what the other alternatives are; in others we do not, except, perhaps, in a vague sort of way. In some cases, we can, from a knowledge of the operative conditions, regard the alternatives as equally likely; in others we have to weight them in the light of experience; in yet other cases we may not be able to value them at all, or only in a very rough manner. The frequency-theory of probability, on the other hand, has really no room for the non-quantitative cases. Even in the modified form, in which frequency is taken to mean the truth-frequency of certain classes of propositions, it appears unsatisfactory. How exactly does it help one to deal with the probability of a judgment, to advise him to ascertain first the probability of the whole class of judgments to which it belongs? Strictly speaking, it is only when we know the probability of a proposition that we know to what class it belongs in respect of truth-frequency, if we can ever know this at all.

§ 5. *The Calculation of Odds, etc.*

The term "chances" is sometimes used instead of the expression "probability." At other times the expression "chances" is used as the equivalent of the term "odds." This last expression is met with more often than the term probability in connection with problems of hazard. By "odds" we mean the ratio of favourable to unfavourable

possibilities, f to non-f. It is always easy to convert probabilities into odds and *vice versa*. For $t = f + \text{non-}f$, so that if we know p (i.e. f/t) we can determine non-f, which $= t - f$, and so we can state the relation of f to non-f, that is, the odds. Similarly, if we know the odds, that is, the ratio f to non-f, we can determine t, which $= f + \text{non-}f$, and so we can obtain p, which $= f/t$. For example, if we are told that the probability of an event is $11/36$ then $t = 36$, and $f = 11$; therefore non-f $= 36 - 11 = 25$, and the odds, therefore, are 11 to 25. Conversely, if we are told that the odds are 11 to 25, then $t = 11 + 25 = 36$, and $f = 11$, therefore the probability is $11/36$. Sometimes the probabilities and the odds given are those *against* the event; but this involves no difficulties whatever, if it is remembered that the probability against the event simply $= \text{non-}f/t$, and the odds against it are non-f to f. The probability against an event must be distinguished from its improbability. The expression "improbability" simply means a low degree of probability. Popularly, the term "probable" often means "very probable," or "more probable than not," and the term "probability" means "a high degree of probability."

It is sometimes easier, or quicker, to calculate the probability against an event than the probability in its favour. In such cases, if we want to ascertain p it is best to determine non-p first and to subtract it from 1. For $1 - p = \text{non-}p$, because $p = f/t$, and $\text{non-}p = \text{non-}f/t$; therefore, $p + \text{non-}p = f/t + \text{non-}f/t = t/t = 1$. Consequently, $p = 1 - \text{non-}p$,

and non-$p = 1 - p$. For example, the probability of tossing head in either of two throws of a coin is, as was shown above, $3/4$. Now, there was some risk of miscalculation in that case, and the risk is much greater in more complex cases. It would have been simpler and safer to argue that the probability *against* obtaining head in either throw is the same as the probability of getting tail in both, that is $1/2 \times 1/2 = 1/4$. Thus, non-$p = 1/4$, and p therefore $= 1 - 1/4 = 3/4$.

§ 6. *The Law of Succession and Induction by Simple Enumeration.*

There is one special type of *a posteriori* calculation of probability that calls for separate consideration, as it bears on the question of induction by simple enumeration. If an event has been observed to occur a certain number of times under certain circumstances which do not appear to be causally connected with it, then, of course, we cannot feel confident that it will always occur under those circumstances; but the more often it has been observed to occur, the more probable will its recurrence under those circumstances appear to be. According to Laplace, each previous occurrence may be regarded as a reason for expecting its recurrence, and each failure is a reason against expecting its recurrence. Now, the question of the next recurrence of the event considered by itself involves two possibilities, namely, its occurrence and its non-occurrence. Of these two possibilities, one is favourable. Accordingly, if an event has occurred

m times and has never been known to fail under certain circumstances, then the probability of its next recurrence, under those circumstances, is $\frac{m+1}{m+2}$, or the odds in its favour are $m + 1$ to 1, so that the larger the number of occurrences observed, when no exceptions are known, the more nearly does the probability of the next occurrence approximate to 1, that is, certainty. Thus, for example, if the sun has been observed to rise and set within periods of twenty-four hours on a trillion occasions, and there has been no exception, then the probability of its rising and setting in the next twenty-four hours is $\frac{\text{a trillion} + 1}{\text{a trillion} + 2}$! which is practically 1, or certainty. But the probability of more than one such recurrence is certainly less. The probability of r recurrences will be $\frac{m+1}{m+r+1}$. Induction by simple enumeration may thus attain to a high degree of probability if the expectations based upon it are confined to a comparatively small number of recurrences. But the probability of a real generalization based upon simple enumeration is never very high. For in a real generalization the number of recurrences contemplated (r) is practically infinite, and consequently the value of $\frac{m+1}{m+r+1}$ cannot be high.

If, under the same circumstances, the event in question has been observed m times and has failed

x times, then the probability of r recurrences will be $\frac{m+1}{m+x+r+1}$. This formula is knows as the Law of Succession, and includes, of course, the preceding formulæ, which are obtained from it if x or $r = 0$, according to the nature of the case. In this fuller form we see the basis of the probability of the recurrence of partial correlations or associations, as ascertained by the statistical method of exact enumeration. What has just been said about the probability of inductions by simple enumeration applies here likewise.

§ 7. *The Use of Calculations of Probability.*

Lastly, of what practical value is the whole calculus of probability? Some people have very exaggerated notions of its practical value. This is partly due to our respect for figures, in consequence of the important place which mathematics holds in modern science. One tacitly assumes that exact figures express precise knowledge, and one has no suspicion of the ill-defined nebulosities that may masquerade in precise ratios and fractions. Partly, however, the exaggeration is also due to the frequency-theory of probability. This theory rather encourages the confusion of probabilities with frequencies. There is, of course, a connection between them. We have seen that probability can, in many cases, only be calculated on the basis of observed frequencies. Nevertheless, probability and frequency are not identical. Now frequencies,

when treated with the necessary precautions, may be of the greatest practical value—as is evident from their use in insurance business and in kindred enterprises. But the calculus of probability is another matter. Probability is concerned even with single events and small groups or series of events, while frequencies always refer to large classes, or long series of events, to what happens "in the long run." The practical difference is obvious, even in roulette. The bank, doing business with large numbers of players, can rely on frequencies which secure it certain advantages. The individual player, limited to a much smaller number of ventures, simply gambles, and usually pays dearly for his estimates of probability, even when these are based on rational calculations, and are not merely the result of inspiration or superstition. The calculus of probability is, of course, secure of its reputation, just like the ambiguous oracles of antiquity. Whatever happens, the calculus is right. Whether you win or lose, whether you have a long run of good luck, or of ill luck, the calculus is equally right. This may console those who cherish their delusions more than their treasure; but sensible people will not put their trust in such ambiguous oracles. There are, it is true, ingenious gambling systems, based on frequencies rather than on probabilities. But even these systems have their day and cease to be. The best of them is but a snare, cheating the fowler of a bird in the hand for two in the bush. Its validity always depends on "the long run," which easily outruns the resources of any ordinary individual.

For similar reasons, even in legitimate insurance business, the company, relying on frequencies, is on a much better footing than the individual client. But the practical exigencies of life often make it advisable for the individual to take high risks for small amounts rather than incur low risks for large amounts—not to mention the benefits which accrue to the community as a whole from insurance organizations. No precision of figures can eliminate the essential uncertainty of the probable. And, in the last resort, the best method of estimating the probability of anything is by a close examination of the actual conditions. Even insurance companies do not rely entirely on frequencies, but have each case examined by an expert (doctor, or engineer, etc.), according to the nature of the risk. This is done primarily in order to determine accurately to what precise class the risk belongs, since each class has its own frequency. But that is not the whole explanation. For, if everything were taken into account indiscriminately, each case would be *sui generis*. There can be no doubt, however, of the great practical, as well as theoretical, value of a knowledge of frequencies and correlations. In comparison with it, the value of the calculus of probability is almost negligible.

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